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THE

SELECT WORKS

OF

ANTONY VAN LEEUWENHOEK.

VOL. II.

THE
SELECT WORKS
OF
ANTONY VAN LEEUWENHOEK,

CONTAINING HIS

MICROSCOPICAL DISCOVERIES

IN MANY OF THE WORKS OF NATURE.

TRANSLATED FROM THE DUTCH AND LATIN EDITIONS PUBLISHED BY THE AUTHOR,

BY SAMUEL HOOLE.

VOLUME THE SECOND. PART THE THIRD.

Hy ſprack oock van de boomen, van den Ceder-boom aen die op den Libanon is, tot op den Ysop die aen den wandt uyt wast: hy ſprack oock van het Vee, ende van het Gevogelte, ende van de kruppende Dieren, ende van de Visschen.

J. Koningen, iv. 33.

And he ſpake of trees, from the cedar-tree that is in Lebanon, even unto the hyſſop that ſpringeth out of the wall: he ſpake alſo of beaſts and of fowl, and of creeping things, and of fiſhes.

I. Kings, iv. 33.

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M,DCCC,VII.

On the formation of the Elm, Beech, Willow, Alder, Ebony, Box, and Lime-tree, with an explanation of the manner in which Pipestaves for making Casks are prepared from Oak-timber.

THE formation of the Elm is represented in Plate XI. *fig.* 1. A B C D, and the size of the piece of wood here magnified, when viewed by the naked eye, is shewn at letter E; this figure also contains as much as the tree encreased in size in the space of one year; A B and C D, denote the places where, in autumn, the growth ceased.

The small perpendicular or ascending vessels which lie in great numbers intermixed among the large ones, are less in this wood than in the Oak, and moreover, each of these small tubes or vessels is composed of tougher and thicker membranes than those in the Oak.

At A B and C D are shewn the horizontal vessels lying lengthwise.

Fig. 2, F F F F, exhibits the horizontal vessels cut transversely; such of them as appear collected in small parcels, I take to be those which are beginning to be formed from the perpendicular vessels; and, that those which are collected together in great numbers, are the produce of several years growth, and contain as great a number as ever would be formed in the future growth of the tree.

G G G G, shew the very small perpendicular vessels lengthwise. H H represents one of the larger perpendicular vessels cut lengthwise down the middle. Upon a more accurate examination of these, we shall perceive them to be composed of exceedingly thin membranes, covered with filaments twisted in a serpentine form, and having the appearance of dark spots and tubercles or risings, as at *fig.* 3, B.

Among this species of tree, we often see some, from which, at the

thick part of the ftem the rifing fap leaks or oozes out, and in this liquor I have often, in the fummer time, obferved various animalcules, but which animalcules I could by no means conceive to have iffued from the wood; but rather, that the firft rudiments of them had been depofited either by the rain or dew.

At *fig.* 4, A B C D, is pictured a fmall particle of Beech, to the naked eye appearing of the fize reprefented at F; the length of this figure alfo fhews the thicknefs acquired by the tree in one year's growth, the beginning and ceafing of which growth plainly appear at the letters A D or B C.

In this wood are two forts of perpendicular veffels, large and fmall ones; and, I am inclined to think, that there are alfo two forts of horizontal ones, very minute, one fort of which appears at E E E, lying in fmall numbers together, and when cut tranfverfely they are fhewn in *fig.* 5, at the letters H H H.

The other fort lie in detached parcels, and are fhewn lengthwife at D C; thefe veffels are alfo very fmall in comparifon with the perpendicular ones, and are compofed of large clufters collected together, a reprefentation of them when cut tranfverfely may be feen in *fig.* 5, at I I.

K K K K, are the large perpendicular veffels cut longitudinally; and thefe I have almoft always obferved to be covered with particles which, viewed by a common magnifier, exhibited the appearance of globules.

Fig. 6, A B C D, is a fmall piece of Willow, to the naked eye appearing of the fize fhewn at F; this wood confifts of two forts of perpendicular veffels, fmall and large; the large ones are covered with particles bearing the appearance of globules, and in thefe I obferved certain oblique ftreaks, which I long ago concluded to be valves*:

* The author's opinion refpecting the probable ufe of valves, in the veffels of trees may be feen in his Effay on the Oak, vol. I. p. 1

one of thefe is fhewn in *fig.* 7, where the perpendicular veffels are exhibited lengthwife, and it may be feen in the veffel marked G G. The fmall perpendicular veffels are compofed of exceffively thin membranes, and thefe it was not in my power to reprefent truly in a drawing.

In this wood I only obferved one fort of horizontal veffels, which are fhewn lengthwife in *fig.* 6, at E E E; the number of thefe is very fmall, compared with the horizontal veffels I have obferved in other woods.

Fig. 7, H H, reprefents them cut tranfverfely, and intermixed with the perpendicular veffels, which in the fame figure appear lengthwife.

In one of the large perpendicular veffels reprefented in *fig.* 7, at G G, I have fhewn the appearance they exhibit of being covered with globules, but thofe exceffively minute.

Fig. 8, is a particle of Alder, about the thicknefs, when viewed with the naked eye, of an hog's briftle: this wood has too forts of perpendicular veffels; the fmaller ones confift of exceffively thin membranes, the larger of membranes covered with particles of wonderful minutenefs, and to which no other figure can be affigned than that of globules.

A B or C D denote the length of the tree's increafe in the fpace of one year; E E E E are the horizontal veffels.

Fig. 9, F F F F, are the large perpendicular veffels cut longitudinally; H H are the fmall perpendicular veffels: G G, are the horizontal veffels, which here are feen cut tranfverfely.

Fig. 10, A B C D E F, is a fmall particle of Ebony, the growth of the ifland Mauritius: this figure is drawn from a microscope, magnifying objects much more than thofe from which the drawings of the other woods were made, becaufe the veffels in this wood could not be conveniently difcovered by a common magnifier. The

particle of wood here ſhewn, in which are to be ſeen about eleven hundred perpendicular veſſels, is not larger than can be covered by a common grain of ſand. I did, at firſt, deſign to have made a drawing of a larger portion of this wood, with intent to ſhew the decreaſe of the growth in autumn, and its renewal in the ſpring; but on reflection I found that ſuch an attempt would be fruitleſs, foraſmuch as this wood grew in a climate where vegetation never ceaſes; the iſland Mauritius being ſituated a few degrees to the northward of the Tropick of Capricorn.

In this wood there are four ſorts of perpendicular veſſels; G G G are the largeſt of them; ſome of which ſeem to have had in them a kind of fluid ſubſtance, which in drying coagulated in ſeveral places, as may be ſeen in *fig.* 11, at K K, where one of theſe largeſt veſſels is ſhewn cut longitudinally.

Fig. 12, is alſo one of thoſe larger veſſels (drawn from a microſcope of ſtill greater magnifying power), much more tranſparent than the former one, and likewiſe with inconceivably minute particles.

The ſecond ſort of perpendicular veſſels, which generally are ſituated between the horizontal ones, are ſhewn lengthwiſe at A B C and H H; theſe, in many places, ſeem partly filled with a black ſubſtance.

The third ſort of perpendicular veſſels lie in the ſame direction with the circumference of the tree, and are repreſented at B E or C D.

The fourth ſort of perpendicular veſſels, which are ſhaped ſomewhat like a lozenge, and are placed among the larger perpendicular veſſels, are compoſed of a much more ſolid wood or ſubſtance than the veſſels in the other woods; for the ſmall round cavity which appears in the face of each veſſel, cut tranſverſely, as appears in the figure, is the only aperture in each veſſel, and the remainder of ſuch veſſel ſurrounding this cavity is ſolid wood, conſtituting the ſubſtance of the veſſel; and this fourth ſort of veſſels are ſo cloſely

compacted and conjoined together, that they ſeem to be one ſingle piece of wood; as if we were to figure in imagination a number of ſmall holes, bored in the moſt exact order and proportion, in a piece of the hardeſt wood.

Fig. 11, I I, are the horizontal veſſels cut tranſverſely: two of theſe may be ſeen lengthwiſe, in *fig.* 10, at the letters A B C and H H. *Fig.* 11, L L, are the ſmaller perpendicular veſſels cut longitudinally.

Fig. 13, A B C D, is a very ſmall piece of Box-wood, drawn from the ſame microſcope as the piece of Ebony before-deſcribed; and, although I examined this wood very attentively, I could not perceive in it any appearance of alteration in its texture, as reſpecting or denoting the different ſeaſons of the year in which it was produced; for which reaſon I made a drawing of no more than one minute particle, here repreſented. I found this wood to conſiſt of two ſorts of perpendicular or aſſending veſſels, namely, a larger ſort and a ſmaller, intermixed with the larger.

The larger veſſels are compoſed of fine membranes, full of particles wonderfully minute, which are repreſented in *fig.* 14, where the larger veſſels are ſhewn, cut longitudinally, as denoted by the letters E E E E.

The cavities of the ſmall veſſels are very much like to thoſe in the ſmall veſſels of the Ebony-wood, and theſe, when cut longitudinally, are ſhewn in *fig.* 14, at F F.

A B, or C D, are the horizontal veſſels, as they appear viewed lengthwiſe, and cut tranſverſely, they are ſhewn in *fig.* 14, at G G.

I have often examined the Lime-tree, as well that which is brought to us from Norway to be uſed in carving wooden figures, as that which grows in this country; and, in order to ſhew the wonderful make of this wood, I have cauſed a drawing to be made

of a ſmall piece, as ſeen through the microſcope; this is ſhewn at *fig.* 15, H I K L.

All the parts of which this wood conſiſts, ſeem to me, to be formed with a cavity in each of them; and, if we reflect on the minuteneſs of the perpendicular veſſels and tubes which are to be ſeen in this ſmall piece of wood, we cannot but contemplate them with admiration, as I myſelf have often done; for the real ſize of this piece of wood, from K to L, is not more than five hairs breadth.

At H I, and alſo at K L, are ſhewn the horizontal veſſels in the Lime-tree. I have obſerved that, in ſome places, they lie much cloſer together than here repreſented.

In *fig.* 16, A B C D, are ſhewn ſome of thoſe veſſels and woody tubes of the Lime-tree, which, in the former figure, appear cut tranſverſely, and are here ſhewn longitudinally.

TO THE READER.

Among the different ſpecies of wood deſcribed by Mr. Leeuwenhoek, he makes no farther mention of the Aſh than by laying down the ſame criterion to judge of its goodneſs as of the Oak. His words are as follows:

"The Aſh timber brought from Norway, which is very ſlow in its growth, is of a very ſpongy texture, and conſequently of a weak and periſhable nature, and by no means to be compared to the Aſh which grows in theſe parts, and in a good ſoil; by reaſon that this wood, as well as the Oak, at the firſt of its growth, every year forms very large veſſels, and the reſt of the year much ſmaller ones, conſequently the quicker the growth, the fewer of the larger, and the more of the ſmaller veſſels will be formed within the ſame ſpace. I have examined many Norway Aſh-trees, and have obſerved that, in the ſpace of twenty, thirty, nay, forty years, they had not acquired an inch in their ſemidiameter; whereas thoſe growing in the neighbourhood of our town would encreaſe an inch in their ſemidiameter for many years ſucceſſively; and I conſider

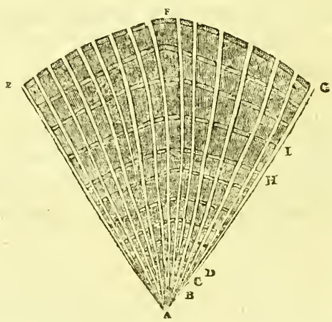
Aſh-timber, growing in warm climates, to be, beyond compariſon, preferable to ſuch as is produced in cold regions."

The Tranſlator in resuming this part of the ſubject, obſerved a paſſage in the Eſſay on the Oak, which had ſomehow eſcaped him; he therefore takes leave to introduce it here.

As many perſons cannot conceive how caſks made of Oak can contain liquors, when they conſider that the wood is compoſed of nothing but numbers of ſmall tubes united, I cannot refrain from giving an explanation of the manner in which Oak is prepared, and worked up in the making of tubs or caſks, which is as follows: The tree is ſplit or cleft lengthwiſe into two, four, or ſix pieces in proportion to its ſize, but ſo that the ſplitting or cleaving may extend from the circumference to the centre of the tree; though when a tree is cleft in two, as I have ſeen performed in much of the Riga timber, pieces are again cleft from the flat ſides of each half, making a ſort of four ſquare ſtaves; likewiſe, when large trees have been thus cleft down the middle, planks or ſtaves are made from them, by cleaving pieces from that ſide which was the middle of the tree; and theſe pieces will be four ſquare and not triangular; by all theſe methods of ſplitting or cleaving the wood, the perpendicular, and alſo the horizontal veſſels will be divided lengthwiſe, or according to the ſame direction in which they were placed in the tree, and they will all lie lengthwiſe in the plank or ſtave, the perpendicular veſſels from end to end, and the horizontal ones from ſide to ſide, by which means the liquor in a caſk or vat, made of timber thus prepared, muſt preſs againſt the ſides of the veſſels or tubes of the wood; and this is the reaſon why caſks made of Oak, though it is formed entirely of tubes, do yet retain the liquor;—to illuſtrate this by an example:

Let A E F G, reprefent a piece of Oak timber, fplit or cleft lengthwife into fix parts, one end of which is fawed tranf-verfely, and planed or fmoothed, as reprefented in the figure. Now if this wood be cleft in fuch manner that A E, A F, or A B C D H I G is made one fide of the ftave, it follows that all the perpendicular veffels, and alfo the horizontal ones which extend from the centre A to the circumference E F, and here are denoted by fixteen double lines, muft be feparated from each other longitudinally, whereas, on the contrary, if the wood was to be cleft from E towards G, then all the horizontal veffels would be cut tranfverfely, and thus lying through the ftave with one of their orifices opening in the cavity of the cafk, and the other on the outfide, the liquor would leak out. And this is the reafon, why if entire trees are fawed into ftaves, all thofe, except what are fawed out of the middle of the tree, will fuffer the water to pafs through them, efpecially if the preffure of the water be very great.

And confidering this, we are not to wonder that new built fhips are fubject to leaks, for if the horizontal veffels have their orifices both on the infide and outfide of the planks, and thefe are not ftopped by fome kind of pitching or dreffing, the water muft leak through.



Various obſervations on the Herring.

I HAVE often reflected on the following circumſtance attending river fiſh, namely, that where they have a plentiful ſupply of food, their inteſtines are always covered with fat; and on the contrary I have observed, that many ſea fiſh, ſuch as Cod-fiſh, Haddocks, Plaice, Flounders and the like, though they are plump and bulky, never have any fat on their inteſtines: Herrings, however, do not come under this deſcription, for, not only their bowels are very fat, but their whole bodies are ſo much ſo, that ſometimes when they are cut, the fat or oil follows the knife, eſpecially at that time when the roes begin to ſwell, at which time they are called in this country *Maatgens-Haringen.*

After much turning this matter in my thoughts, I had a fancy to know what is the food of this fiſh, and for that purpoſe I enquired of many men, uſed to that fiſhery, what food they generally found in the ſtomachs of Herrings when firſt caught, but the conſtant anſwer I got from them was, that they never found any. At length I met with a merchant who fits out ſhips for the Herring fiſhery, and from him I learned, that in a certain tract of ſea near the coast of Scotland, Herrings are caught, in the ſtomachs of which are found ſome kind of ſmall fiſhes, but that thoſe Herrings will not keep long.

Not content with this, I determined to wait for the ſeaſon, when certain Herrings are brought to our town, which, as I have heard, are caught in great numbers not far from Amſterdam, in that bay or ſea, which, among us, is called by the name of the *Zuyder Zee.**

* The *Zuyder Zee* or *Zudder Zee*, a ſmall Mediterranean ſea between North Holland and Frieſland, whence probably its name of Zuyder, or Southern ſea, being ſo ſituated in reſpect of North Holland. It is very ſhallow, and by Sir William Temple in his remarks on the United Provinces, ſuppoſed to have been formed by ſome great inundation in the middle age, or about the ninth century.

Theſe Herrings being brought to our town for ſale about the middle of March, I diſſected ſix of them, and I found in that ſubſtance which I took out of one of their ſtomachs, three living worms, all of the ſame ſhape, and ſomewhat thicker than an hair; but, about four hairs breadth diſtance from their heads, their bodies, which in that part were round, were about four times thicker.

This Herring had been dead at leaſt twenty-four hours before I examined it, and perhaps theſe worms might have lived a long time, if they had remained in the chyle and within the stomach.

Moreover, in theſe worms or animalcules, I ſaw a vein, bent in a ſerpentine form, and of a blackiſh colour, lying the whole length of the animal, and near this vein certain ſmall round particles driven to and fro with great ſwiftneſs, affording me a very pleaſant ſpectacle.

Another of the Herrings had nothing except a certain red matter in its ſtomach, which, upon viewing it by the microſcope, I thought I ſaw to conſiſt of certain round particles, which were almoſt ground down by the action of the ſtomach; and I ſaw that they had been compoſed of many veſſels: this was alſo the appearance of the white chyle which I took out of the ſtomachs of all theſe Herrings.

Seeing theſe things, I did not wonder that fiſhermen ſhould imagine Herrings have no food in their ſtomachs, becauſe Herrings do, in my opinion, feed on ſuch ſmall fiſhes, that they cannot take in ſufficient quantities of them to diſtend their ſtomachs, as we ſee in other fiſh; and hence it is ſaid, that Herrings have no food within their ſtomachs.

Now, ſince we find ſo ſmall a portion of food in the ſtomachs of Herrings, and yet that they are ſo fat at the time, when, as I ſaid before, their roes are yet very ſmall, we muſt conclude that Herrings are continually feeding, and that their food consists of ſuch minute fiſhes as eſcape the eyes of the fiſhermen, whereas other fiſhes are accuſtomed to cram their ſtomachs with larger fiſhes, and to ſuch a degree, that ſometimes they are diſtended to a fifth or ſixth part of the fiſh's own bulk; and therefore it is, that the fiſhes

they so ſwallow, remain in their ſtomachs ſeveral days, until their bones are by the proceſs of digeſtion ſo comminuted, as to be fit for admiſſion into the inteſtines.

Since then we ſee, that Herrings have ſo little food at a time in their ſtomachs and inteſtines, and yet become ſo very fat, which I think is owing to their taking abundance of nouriſhment, we muſt conclude, that there are more animalcules or minute fiſhes in the ſea, than has ever yet been thought of: and hence we are not to wonder that Herrings are ſometimes caught in one, and ſometimes in another part of the ſea; ſometimes in the ſhallows, and ſometimes in the deep water, according to the places where the ſmall fiſhes on which they feed do from time to time reſort; ſo that, here I think, the words of ſcripture may be applied "Whereſoever the carcaſe is, there will the Eagles be gathered together."

About the end of March, ſome more of the ſame ſort of Herrings were brought to our town for ſale; ſeveral of theſe I opened, and ſaw that their livers were as large in proportion to their ſize as thoſe in other fiſh, which livers were alſo provided with a gall-bladder; theſe Herrings were in that ſtate, that many of them were deſtitute of roes. In examining the chyle which I took out, both from their ſtomachs and inteſtines, I perceived no difference, except by obſerving in ſome of them a few round particles, which I deemed to be eggs of fiſh; and of this I was the more aſſured when, upon breaking the membrane in which theſe round particles were encloſed, and which was very tough, I ſaw the contents to be of a fluid nature, mixed with larger and ſmaller globules. Several of theſe round particles I ſeparated, as well as I could, from the chyle, and placed beſide them ſome ſingle grains, or eggs, which I had taken out of the hard roes of other Herrings, and then I clearly found that thoſe globules which I had taken out of the ſtomach and bowels, were Herring's eggs; whence, I concluded, that thoſe Herrings which, about this ſeaſon, are ſometimes caught in the Zuyder Zee, in such quantities, that they are more than ſufficient for conſumption, will, for want of other food, devour their own eggs or roes.

In the next place, I took out of the cavity of the belly of two Herrings a white worm, about as thick as a horſe-hair, whoſe body, at each end, eſpecially near the tail, was very thin and pointed. This ſpecies of worm I have found in the ſtomach and bowels of ſeveral kinds of fiſh. Theſe worms, I concluded, had forced their way through the ſtomach or bowels, and thus had got into the cavity of the belly, becauſe their bodies were of a very hard texture.

The circumſtance, that theſe worms ſhould penetrate, through the inteſtines into the cavity of the belly, may ſeem wonderful to many perſons, who cannot conceive how a wound can be made by a worm in any inteſtine, and yet the chyle not run out of that wound into the cavity of the belly. But it ſhould be conſidered that the head of this worm (which is very ſharp), piercing through the inteſtine, would cauſe little or no injury to the part, becauſe neither the head or body make much of a cut or wound, and, therefore, the veſſels in its paſſage would only be ſtretched aſunder, and, immediately after it had paſſed, cloſe again.

A gentleman of my acquaintance, hearing me argue in this manner, told me, that he kept, in an encloſed place at his houſe, a parcel of hens, in order to ſupply himſelf with eggs, and that his maid ſervants were accuſtomed to throw the ſand and ſweepings of the chambers into this place, without taking the trouble to pick out ſuch pins as might be in them, and that the hens, which were kept thus ſhut up, finding no ſmall ſtones or pebbles to ſwallow, as it is in their nature to do, will pick up the pins. Theſe fowls are generally killed in the winter-time; and, my friend told me that, upon cutting them up, himſelf and all his family, not excepting his men and maid ſervants, had ſeen ſeveral pins ſticking in their breaſts; and that, alſo, a pin was found which had come out from one of their bodies through the breaſt-bone; and that another pin was found in the bottom of the breaſt, with its point downwards, ſticking out of the ſkin, ſo that only the upper part of the pin remained between

the ſkin and fleſh, and two pins ſtuck to the fleſh in the middle of the ſtomach, which had penetrated through its coat; and, within the ſtomach, were many pins mixed with the food, which, probably, if the fowls had remained alive, would have found a paſſage out in ſome part or other. This being the caſe, it is no wonder that a worm ſhould find a paſſage, not only into the cavity of the ſtomach, but into other parts of the body.

On the 14th of April more Herrings were brought to market; when I obſerved many eggs ſcattered among them, ſo that I concluded for certain, that, at this time, moſt Herrings depoſite their eggs or roes.

Again, I took ſeveral Herrings, and, from the appearance of their inteſtines, I judged they had been longer out of the water than thoſe I formerly examined; yet, in the inteſtines of one of theſe, I ſaw a living worm.

Moreover, I took, out of the bowel of a male Herring, the ſoft roe of which was grown to its full ſize, ſeveral Herring's eggs, which were in that part of the gut near its orifice; from whence it again appeared to me, that the ſhell or membrane of theſe eggs is ſo tough and hard, that it cannot be digeſted or diſſolved, either in the ſtomach or inteſtines.

I alſo found, in this inteſtine, many particles not of animal, but vegetable ſubſtances, and, among them, a hollow piece of a ruſh, or ſome ſuch vegetable; in which I could very plainly perceive, the pores or tubes of which it was compoſed, and alſo ſome exceedingly ſmall flat particles, alſo conſiſting of tubes, joined together. I likewiſe found ſeveral thin oblong particles, the formation of which could not accurately be diſtinguiſhed, they being impervious to the light; and, among other things, I ſaw the ſhell of a ſea-ſnail, not larger than a grain of ſand, whoſe circumference was divided into eight parts or joints. The ſhell of this ſnail was ſo very thin, that when the moiſture it contained was evaporated, it entirely loſt its handſome ſhape. And thus it was manifeſt to me,

that Herrings not only feed on ſmall fiſhes, and even on their own eggs, but that, when urged by hunger, they will ſwallow any thing they meet with.

Since then we ſee, that the ſmaller fiſh, and eſpecially Herrings, which take ſo little food at a time, that it is the vulgar opinion, becauſe their ſtomachs are not diſtended like thoſe of other fiſh, that they live entirely without food, do yet become very fat; when, on the contrary, cod and other fiſh, which ſometimes have, at one time, as many as six ſhell-fiſh in their ſtomachs, have, nevertheleſs, no particles of fat, either between the fleſhy parts, or in the inteſtines, except the liver: What, I aſk, is the concluſion to be drawn from hence? Surely (with ſubmiſſion to better judgment), no other than this: that thoſe kind of fiſh have no veins or veſſels adapted to ſecrete the fat from the food which is digeſted in the ſtomach or inteſtines, and much leſs any veſſels to ſecrete the fat between the fleſhy parts.

But ſince cod, haddocks, and other fiſh of the like kind, are deſtitute of veſſels to ſeparate the particles of fat, and yet are always able to get a large ſupply of food, ſuch a quantity of nouriſhment is conveyed to the fleſhy parts of their bodies, that they become ſo hard and dry (I mean at the time when their roes firſt begin to ſwell, which is in the ſummer), that they are quite inſipid to the palate, and, therefore are ſaid to be out of ſeaſon, whereas, it ſhould more properly be ſaid, that they are then too much fed, or have taken too much nouriſhment; but, when the roes in theſe fiſh begin to increaſe, ſo that in a few weeks, they muſt be depoſited, ſo much of the nouriſhment paſſes into the roes that the hardneſs of the fleſhy parts is diminiſhed, and then thoſe fiſh again become taſteful to the palate, and are, therefore, ſaid to be in ſeaſon.

Now, as the larger fiſh are fed and nouriſhed by devouring the ſmaller ones, I have often thought, whether minute cod-fiſhes, or other fiſh, when firſt come forth from the eggs, may not feed on thoſe exceeding minute fiſhes, or animalcules, which are found in

incredible quantities in all waters, and are ſo minute, that millions of them are ſcarcely equal to the ſize of a grain of ſand; and, when we conſider that the young fry of pearch, trout, jack, and the like, when only a few days or weeks old, are always obſerved ſwimming ſlowly againſt the ſtream, we may conclude, that it is with intent the better to catch thoſe very minute fiſhes, or animalcules, which are carried down the ſtream in the contrary direction, and to feed on them, till they are of ſufficient growth to prey on larger fiſh; and, if we do but obſerve the very large eyes of thoſe ſmall fry, much bigger in proportion than other parts of their bodies, we may form a tolerable conjecture, that in the firſt creation, they were appointed to have ſuch large eyes, for enabling them the better to diſcover and catch thoſe minute fiſhes which ſerve them for prey.

I have formerly laid it down as a fact eſtabliſhed*, at leaſt in my opinion, that fiſhes living in rivers or very deep waters, or ſuch waters as are always in motion, are not obnoxious to any decay or diſeaſe, and never die of old age; and now, to apply that idea to the ſubject under conſideration. I have examined the ſcales of Herrings, and I uniformly drew this concluſion, that all the Herrings caught by our fiſhermen were, as far as I could obſerve, barely a year old; in confirmation of which opinion, upon inſpecting Herrings caught about the beginning of the month of June (which were procured privately, or as it were by ſtealth, becauſe, by our laws, Herrings are not then permitted to be taken), they being ſometimes ſent to me as preſents, I always found them to be of a much ſmaller ſize than thoſe caught a month or two later; and I have thought that perhaps all Herrings caught in the North Sea iſſued from the roes or ſpawn which had been depoſited the preceding year. Now, if we lay it down as a certain fact, that all the kinds of fiſh to us known when they firſt begin to lay their eggs or ſpawn, are not a year old, and that the greater part of thoſe Herrings, which are found on theſe coaſts in any one year, depoſite the eggs from whence iſſue all

* See vol. I. p. 71.

the Herrings caught in the ſucceeding year, (for if it were not ſo, there would certainly be found in the ſhoal ſome Herrings much larger than others, as we may obſerve in all kinds of fiſh which have ſcales); we may from thence draw this concluſion, that when Herrings have depoſited their ſpawn, they do immediately, or in a very ſhort time afterwards, quit theſe ſeas, and never return thither.

About twenty years ago, I heard a ſtory of an uncommon large Herring, which was to be ſeen in our town, and, for the novelty of the thing, was given a preſent to a company of great perſonages at a public dinner. This Herring, I think, very probably had wandered from the ſhoal, or had by ſome means been detained in our ſea.

If then, we obſerve that in the ſea adjoining to us, no Herrings are found which are more than a year old, and that they then entirely quit our coaſts, and none of them are caught the following year; who can aſſign any other reaſon for this, than that Herrings, when grown large, require more food for their growth and ſuſtenance than they can get in our ſeas, and therefore they migrate, or reſort to thoſe places where they find a greater or more ſolid ſupply of food.

To conclude, the ſecrets of Nature, reſpecting fiſhes in the ſea, as well in regard to their resorting to certain places, as in their departure from thence, cannot be investigated with any preciſion: for inſtance, what ſhall we ſay of the fiſh called the Shad, (in Dutch *Elſt),* for thoſe fiſh, when the time of their propagation is at hand, are found in our rivers; but when that time is paſt quit the rivers, nor are ever afterwards, as far as I have been able to learn, caught in the ſea.

Of the ANT*.

IN a Treatiſe, lately publiſhed in Germany, on Microſcopical ſubjects, the author has given drawings of the Ant and its egg, in the latter of which, he has repreſented the young Ant within the egg, though not more than an eighth part the ſize of a full grown Ant: but as theſe figures appeared to me, to be very inaccurately taken, I determined to enter upon an inveſtigation of the ſubject myſelf.

Upon digging up an Ant's neſt, in my garden, and examining the eggs (as they are commonly called), I found them not only to be of different ſizes, but alſo ſome of the young Ants within them, were of a white colour, and lay, as it were, motionleſs, until they arrived at their full growth. Hereupon I began to form very different ideas from the opinions hitherto entertained, reſpecting theſe creatures; and, I concluded, that the Ant, as well as the Weevil and other minute animals (in theſe cold regions), does, in the winter ſeaſon, lie without motion, and does not take any nouriſhment; and that the collections of food which Ants are obſerved to make, and to heap together in their neſts, during the ſummer ſeaſon, is for no other purpoſe than to feed their young.

Theſe my ſentiments may appear new and ſtrange to many; but how can we conceive the egg of any creature to be larger than the hind part or belly of the animal from which it proceeds, and,

* This Eſſay was written in the year 1687. As the Author herein combats ſome generally received opinions, reſpecting the Ant, the Tranſlator has given his own words, as nearly as poſſible, and in the order in which they ſtand, that the Reader may form his own opinion upon the ſubject.

that in time, it can become as large as the whole body of the parent, before the young one comes forth, and that, when come to this growth, it ſhall not afterwards increaſe in ſize? This, however, if theſe are the real eggs, we muſt conceive of the Ant; but, whether the ſame takes place in any other creature I know not, nor have ever obſerved. In all probability theſe eggs, as they are called, when firſt laid, are very ſmall, and continually increaſe in ſize; the concluſion is, that they muſt conſtantly receive ſome kind of nouriſhment to cauſe ſuch increaſe; whence we may infer, that the collection of food which Ants, in the ſummer time carry to their neſts, is, principally, to feed their young. This, my opinion, I doubt not will be greatly controverted, becauſe the young Ants lie incloſed in a kind of ſkin or membrane, and, conſequently, it is difficult to conceive how they can receive nouriſhment from the father or mother, when thus completely ſeparated from them. And yet it is ſtill more difficult to conceive, how an animal in the egg, and alſo the egg itſelf, can daily grow larger, without receiving any nouriſhment. I did indeed, at firſt, imagine that the young Ant was completely formed in this egg; but at length I rejected that opinion, determining to examine the truth of this matter, and, if poſſible, to diſcover by what means theſe eggs of the Ant do increaſe in ſize.

And, recollecting, that I could not be at a loſs for Ants' neſts in my garden, having been formerly much infeſted by them, I took a ſpade full of earth, mixed with Ants and their eggs, which I put on a clean ſheet of paper, and ſat myſelf down before it, to examine the Ants carefully, who are accuſtomed, when their neſts are diſturbed, to carry away their eggs. Many of theſe I took from them, and obſerved, that what moſt of them were carrying was no other than a young Ant, quite white, and without motion; though in others of theſe Ants the white colour was turned to a red. Others were white, and of an oblong ſhape; and theſe I took to be what are uſually called the Ants' eggs. Theſe laſt were ſmaller and ſmaller, and ſome of them no bigger than a common grain

of ſand: but, at firſt, I had beſtowed my attention only on thoſe particles which I had taken from the Ants, conceiving them to be of uſe to me in my enquiry.

To this end I had brought with me ſeveral new glaſſes, in which I collected ſome of each of the before mentioned ſorts, as far as I was able, and, as I had only my ſpectacles with me, but which magnified objects conſiderably, I examined, after the Ants had almoſt all run away, the earth (which conſiſted of clay and ſand mixed together), and found in it ſeveral very ſmall white particles, which, I concluded, had been left there by the Ants, mixed with ſome grains of ſand.

Upon my return home, I examined all with the microſcope, and found, that thoſe which approached neareſt to the likeneſs of Ants, had not aſſumed their perfect figure, though of the full ſize of an Ant; and the only difference conſiſted in this, that they were entirely white, and without any motion, and their claws and horns lay diſpoſed in regular order, in like manner as we obſerve in caterpillars, previous to their change into flying inſects, though theſe, of which I am now ſpeaking, were not incloſed in any membrane. Theſe are, doubtleſs, what are generally called Ants' eggs.

Others of theſe white particles, which were alſo of the ſize of an Ant, and approached nearer the ſhape of an egg (of which ſort I found many), I perceived to be ſhort thick maggots, having in their bodies a black ſpot, which, probably, moſt perſons would conclude to be the young Ant within. Of theſe I carried many about with me in my pocket, and obſerved, that before their change approached, they purged off this black matter, which was their excrements. I ſaw ſome of them, within twenty four hours, and others within forty eight hours, put off their ſkin, and change into a white cryſalis.

Others of theſe particles, or eggs, were ſmaller and ſmaller, till ſome of them I found leſs than a grain of ſand; and, among the ſmalleſt of the particles which I had taken from the Ants, or found

among the earth of their neſt, I diſcovered ſeveral eggs ſo minute, that I could not, with my naked eye, diſcern what they were.

One of theſe eggs I placed before the microſcope, and cauſed a drawing to be made of it, which is ſhewn in Plate XII. *fig.* 1, A B C. And, to give an idea of the minuteneſs of this egg, I meaſured it by a rule divided into aliquot parts; and, I muſt ſay, that ninety of the diameters, breadth or thickneſs of this egg, would not amount to an inch in length.

Another egg I placed before the microſcope, in which the maggot was ſo far grown, that with its head, it advanced beyond the ſhell of the egg. This is to be ſeen at *fig.* 2, D E F; in which figure, E F denotes what had been a part of the yolk of the egg, but was now tranſparent, and only filled with air.

The next day after I had turned up this Ant's neſt, as before mentioned, it rained very hard, and the day following, it was very fair weather; whereupon I went to viſit the place, and found two holes newly made in the earth, and the Ants running in and out. I took up a ſpade full of the earth where theſe holes were made, and found in it many eggs of the ſame minuteneſs I have juſt mentioned: theſe I put into different glaſſes, ſome of which I carried in my pocket; and, in ſome of them, after the ſpace of twenty four hours, I perceived the maggot to be perfectly formed in the egg; but, in the greater part of them, I found the maggot imperfect, being dead, and the egg dried, with the ſhell contracted. I cauſed a drawing to be made, from the microſcope, of one of theſe laſt named eggs, becauſe, in it, the limner could diſtinguiſh all the parts of the maggot's body. This is ſhewn at *fig.* 3, G H I.

I alſo placed before the microſcope one of the maggots, which, at the ſame time, had crept out of the egg, and cauſed a drawing to be made of it as it then appeared. At *fig.* 4, K L M, is the head, and M N K, the body of this maggot.

Upon attentively obſerving this maggot, I was much gratified with

the ſight of an inceſſant motion appearing within its head (and, if I may be allowed the expreſſion, in its brain), for this motion was as regular as we can imagine, juſt as if we were to ſee the motion of the lungs of any animal in reſpiration.

Sometimes I ſaw one of theſe minute maggots open its mouth, as if in queſt of food, at the ſame time producing a minute globule of air at its mouth, which, paſſing through the head, went into its throat.

Moreover, I cauſed one of theſe maggots to be drawn when ſo far grown, that its body was the tenth part of one full grown, and which is repreſented at *fig.* 5, O P Q R ; in which figure, P Q is the head, and the reſt the body. Theſe maggots, when young, lie in a curved poſture, but, when almoſt full grown, they aſſume a ſtraight figure. This maggot was drawn from a microſcope, magnifying objects much leſs than that from which the former figures were taken; from whence the minuteneſs of theſe eggs may very eaſily be conceived. I could have given a drawing of a full grown maggot from the microſcope, but I think it unneceſſary, becauſe, from the laſt figure, we can eaſily form a judgment of a maggot, of the ſame ſhape, when ten times larger.

I muſt confeſs, that I formerly thought no otherwiſe than that thoſe maggots to which, hitherto (for want of knowing better, we have given the name of Ants' eggs), as ſeen by the naked eye, were, in reality, the eggs of the Ant, and that the black ſpot which we ſee in their bodies was the minute young Ant, not yet brought into life. But I cannot ſufficiently wonder, how any one can ſuffer his imagination ſo to deceive him, as to fancy, when uſing a microſcope, that, through it, he ſees in this egg (as we call it) a young unformed Ant. And not only ſo, but to make a drawing repreſenting its body, head, claws, and eyes; when, on the contrary, no perſon whatever can ſee the leaſt appearance of any ſuch in this maggot which, hitherto, has been called the egg.

I have ſeen ſome of theſe maggots, while very ſmall, to have the

dark ſpot before mentioned, and others much larger, and even perfectly formed, not to have the leaſt appearance of it. And, to inveſtigate the true nature of this dark ſpot, which ſome are ſo ſimple as to imagine is a young Ant, I opened the maggot, and ſaw moſt plainly that it was the animal's ſtomach, and that the darkiſh appearance was cauſed by the food in the ſtomach; and, with regard to thoſe maggots which have not ſuch a ſpot, this is owing to the different kind of food brought to it by the parents. In this diſſection I ſaw not only the ſtomach, but the inteſtines adjoining to it, which were filled with globular particles of a darkiſh colour.

How is it poſſible for us to ſee in a maggot (which we call an egg) a very minute young creature? for I have before ſaid, that the whole and entire maggot is transformed into an Ant, ſaving only its ſkin which is left behind; but this ſkin or cuticle is ſo thin, that it does not amount to an hundreth part of the maggot's ſubſtance.

This maggot has on its body, except the belly, many hairs, and is uncommonly ſluggiſh in its motions, ſo that it is ſeldom ſeen even to ſtretch itself out, or to contract its body, but it often moves its head, and ſometimes, though not often, opens its mouth. It is, in a word, entirely unable to ſeek its food, and, as I may ſay, lies immoveable in its neſt, without ever changing its place, and therefore it is neceſſary that the parent Ants, or ſome others of the tribe, ſhould continually provide food and bring it to the young ones.

This being the caſe, the commonly received opinion muſt be done away, reſpecting the induſtry of Ants in carrying proviſions to their neſts for the purpoſe only of laying up a ſtock of food for winter; whereas on the contrary, the greateſt part, and who can tell whether or not the whole, is laid up for the maggots which proceed from their eggs (and who cannot poſſibly feed themſelves), and to provide them with nouriſhment until they are of ſufficient growth to put on the ſhape of Ants.

And, in like manner as I have ſaid, that the Weevil does not lay many eggs at a time, like the Silkworm's moth, and the common

fly, ſo it is with the Ant, which is a long lived animal, and during the whole ſummer continues to lay its eggs (at leaſt in my opinion), and if ſo, they muſt be continually employed in providing food for their young. And this is an innate impulſe or inſtinct implanted in them by Nature, for otherwiſe their young (which are maggots) muſt be ſtarved for want of food.

I am aware that what I have advanced, and theſe my obſervations will be diſputed by many, and the rather, as ſo much has been written of the induſtry and foreſight of Ants in laying up ſtore for the winter, but this I do not at all regard. I freely ſpeak my ſentiments as they occur to me; and I think it is not at all improbable that Ants throughout the winter, and eſpecially if the cold be very ſevere, can live without nouriſhment, lying motionleſs under ground as other ſmall animals do in the open air, ſuch as the flea, the weevil and others. And if we do but obſerve the Ant, when in autumn they devour our grapes, and take notice, if it be cold, how ſlowly they move, we may very well conclude that, when the cold increaſes they may be entirely inactive during the middle of winter.

Some perſons would have us believe that Ants bite the human ſpecies, and that their bite is venomous: but this is what I could never obſerve; and it ſeems to me very plain, that the mouth of an Ant is not made for any ſuch purpoſe, partly becauſe its pincers, which are provided with points or teeth, and are ſituated in the fore part of the head at the opening of the mouth, cannot exert ſufficient force to penetrate through the external ſkin of our bodies, and make any impreſſion on the ſenſitive cuticle; for the pincers are too ſhort, and the teeth in them too many in number, for ſo ſmall an animal to have the power of reaching with them the ſenſitive cuticle; and in the next place, if we do but inſpect all the parts of an Ant's mouth, when in motion, we ſhall ſee that they are not of a nature calculated to do hurt to any animal.

I have often ſeen an Ant produce out of its mouth four oblong particles; but they are provided with ſmall joints like thoſe in the

horns of the Ant, ſo that the pincers or teeth of this animal ſeem deſigned for no other uſe then to grind its food, and alſo to drag the proviſions it collects for its young to the place where the young maggots lie. But I always obſerved, and without any one inſtance to the contrary, that theſe Ants have a ſting which is placed, not in the head, but in the hind part of their bodies, and, if at any time, we moleſt or hurt the Ant, it immediately puts forth its ſting (which at other times it keeps within its body), and continually endeavours to ſtrike, for it does nothing but bend its tail downwards, and inceſſantly moves its ſting in and out of its body.

This motion of the ſting, is ſo natural to the Ant, that though the hind part of its body be cut off from the reſt, the ſting will continue to move for ſome time. And the Ant not only gives us pain merely by ſtinging, but with the ſting it emits a tranſparent liquid which is tranſmitted to the end of the ſting; and, from this liquid, I conceive the principal part of the pain proceeds, owing to the ſharp ſaline particles it contains, and that from hence alſo proceed thoſe humours or ſwellings upon the ſkin which we experience on being ſtung.

The ſwellings or pimples which I experienced from the Ants, in my preceding obſervations of their manner of generation, gave me more pain than in my life before I had experienced from them. For it is almoſt twenty years ago that, having been violently ſtung by Ants, I then, upon examination, diſcovered that they carried a ſting in the hind parts of their bodies.

Although this liquid emitted by the Ant, is in very ſmall quantity, I tried all means I could deviſe to diſcover, if poſſible, the kind of particles it might contain, but I could obſerve nothing, except that this liquor (though very tranſparent and fluid), conſiſted of parts ſo fixed that it would not evaporate; in like manner as if it had been an oil: in one place I fancied I ſaw ſome particles lying, of the nature of ſalts, with ſome others, ſo minute, that I could not aſcribe to them any particular ſhape.

The ſting of the Ant has not any aperture at its extremity, through which this noxious liquid is emitted (as I have heretofore ſaid is the caſe with the Scorpion), but it is provided with a cavity, or groove, ſimilar to what we ſee in thoſe inſtruments uſed by ſailors to wet their ſails, in order to make them hold the more wind. Through this groove, or channel, the Ant is able to emit the liquid, and that as far as the extremity of the ſting.

Upon my taking off this liquid from the ſting, I found that the animal's ſupply was not exhauſted; for, when it drew in the ſting, a freſh quantity of liquid came again forth with it, and this, not once, but ſeveral different times. This gave me reaſon to gueſs the cauſe of an uncommonly large tumour I once had, in my arm, from the ſtinging of an Ant, which was, that the Ant, having crept up my ſhirt ſleeve on the inſide, and there begun to ſting; before I could ſtrip off my other clothes, as well as my ſhirt, it had ſtung me, ſeveral times, ſucceſſively, from whence a large quantity of that noxious liquid had been injected, cauſing a larger ſwelling, and a greater degree of pain.

Fig. 6, B C D, repreſents the ſting of the Ant. A B D E denote part of the animal's tail, which is covered with hairs.

What I have here noted is, however, only to be underſtood of the red coloured Ants, and theſe are the only ſort of them which I ever ſaw in my garden; but, in other gardens, I obſerved another ſpecies of Ants, of a blackiſh colour, and ſomewhat leſs in ſize than the red Ants, and not nearly of ſo hardy a make; for I could eaſily compreſs or ſqueeze the hinder parts of theſe Ants, which part of the body, in the red ones, was very hard. And, whereas I have ſaid, that I always found the former Ants provided with a ſting, I could not diſcover any ſting in theſe laſt mentioned ones, although I opened more than twenty five of them.

As to the maggots in the neſts of theſe black Ants, they did not differ from thoſe produced by the red Ants, except in being ſomewhat ſmaller than the red, when full grown. And, whereas

the firſt mentioned maggots changed into the form of a cryſalis (of which I ſaw great numbers, lying among the maggots), I could not, among theſe laſt maggots, diſcern a ſingle cryſalis; but I ſaw, among them, a great number of what are called Ants' eggs, which, at one end, had a black ſpot. A parcel of theſe I brought home with me; and, upon opening them, I found, in ſome of them, a perfect Ant, of a colour inclining to black, in others of them, an Ant quite perfect, but white.

The ſhell, or covering of this Ant, did not appear to me, to be either a membrane, or the ſkin of the maggot, which it had ſtripped off in undergoing its change, for it was quite ſmooth, and without any wrinkle. And, as I could not imagine how this covering was formed about the Ant, I put ſome of the moſt perfect maggots into a clean glaſs; and, when they had been in it for two days, I found them to differ much from the firſt mentioned ones. For theſe latter ones had begun to ſpin, as ſilk worms do, and, when the thread was about as long as two hairs breadth, they faſtened it, with their mouths, to the threads they had already ſpun (doubtleſs, by means of ſome glutinous matter proceeding out of their mouths); and, in this ſpinning, I did not perceive them move more than their heads and the two joints of their body next the head: and, after two days, they had proceeded ſo far in their ſpinning that the maggot, within the caſe, was no longer to be diſcerned. When theſe had remained in the glaſs in my cloſet, the ſpace of four days, I obſerved, that ſome of the Ants in them had gained ſo much ſtrength as to break through the web or caſe in which they lay, and run about the glaſs.

Hence it alſo appeared to me, that theſe laſt maggots, in their change into a cryſalis, or an Ant, not only at once aſſume all their limbs, and the complete ſhape of their bodies, but alſo that, after this change, they never grow any larger, unleſs they undergo a farther change, and become flying inſects. So that we ſee the error of thoſe who ſay, that, in what they call the eggs of Ants, they

can diſtinguiſh the young Ants when not perfectly formed, and which, in proceſs of time, increaſe in ſize; when, on the contrary, no Ant, or rather its maggot, is incloſed in its web or caſe, until it has attained its full ſize.

This being ſo, it appears, that the young Ant, whether a maggot, a cryſalis, or a maggot in its web or caſe, has, hitherto, been improperly called by the name of an Ant's egg; for, in like manner as we can, with no propriety, denominate the cone or caſe wherein a ſilk worm is incloſed, a ſilk worm's egg, it is equally improper to call the maggot, cryſalis, or the web of the maggot, wherein a young Ant is incloſed, an Ant's egg. For, upon comparing the true ſize of an egg, laid by the Ant, with that of the web or caſe wherein the maggot, containing the young Ant, is incloſed (and which is commonly called the Ant's egg), I will venture to ſay, that nearly an hundred and ſeventy five Ants' eggs do not more than equal the ſize of the web or caſe ſpun by the maggot, and in which it is changed into an Ant.

I have before ſaid, that, at one end of the web or caſe ſpun by the young Ant, there is a black ſpot, which, upon examination, I judged to be the excrement of the maggot, and which it purged off when it was about being changed into an Ant.

This web or caſe, ſpun by the maggot, is of ſo cloſe a texture that one can ſeldom diſcern the Ant within it, unleſs when it is ſo far advanced in its growth that it begins to aſſume a black colour, or when it moves itſelf within the covering or caſe.

In all the Ants' neſts that I have examined, I have obſerved ſome of the Ants to have wings; but in none of the neſts have I been able to find the leaſt traces of the proviſions which they are ſaid to ſtore up; ſo that I am ſtill more aſſured that, in the ſummer ſeaſon, the Ants have enough to do to collect and carry into their neſts a ſufficient quantity of food for the ſupport and nouriſhment of the multitudes of their maggots, both great and ſmall, with which their neſts are, in many places, quite filled.

Moreover, about the middle of the month of Auguſt, I examined ſeveral black Ants' neſts on the Lands we call *Duynen* or the Downs, as well in the neighbourhood of Haerlem, as elſewhere, but I did not find in them any of the maggots from which theſe Ants were bred, nor any of the webs ſpun by them, but only ſome empty webs or caſes, out of which the Ants had crept.

I have alſo ſeen, in ſome neſts, the greateſt part of the Ants changed into flying animals; and, among theſe winged Ants, ſome were eight times larger than the common black Ant.

I, afterwards, again examined the neſts of the red Ants in my garden, and found in them Ants, with their eggs, the maggots, and cryſales, in as great numbers as before.

I alſo placed ſeveral grains of wheat before the Ants' neſts, thinking they would not have ſtrength to carry them away, but I did not obſerve them at all diſpoſed to touch the wheat, and they only carried into their neſts a ſmall barley-corn.

I have thus in my reſearches diſcovered two ſpecies of Ants; and doubtleſs there may be many other ſpecies of them in theſe parts, which have not come under my obſervation. As to what ſorts of Ants are found in other countries, which are much larger than ours, and what may regard their collecting proviſions, and the nature of their domeſtic œconomy, all theſe things are unknown to me.

Theſe are my obſervations (ſuch as they are), reſpecting the Ants, from which I have collected, and ſatisfied myſelf of, the following particulars: Firſt, that Ants lay very minute eggs; ſecondly, that from thoſe eggs maggots are produced, which are fed by the Ants; and that the food which Ants are continually carrying to their neſts in the ſummer time, is for no other uſe than the nouriſhment of thoſe maggots: and thirdly, that what people call Ants' eggs are either maggots, cryſales, or the webs ſpun by the maggots, (with maggots in the inſide).

To ſhew the true ſize of our Ants, I cauſed a drawing to be made of ſome of the red ones, which is expreſſed at *fig. 7*.

After this, being about two hours' journey from our town of Delft, and going into a plantation, belonging to a gentleman's houſe, a working man ſhewed me ſeveral Ants' neſts, namely, ſome of the red Ants of the ſame ſize with thoſe in my garden, in which I found a great number of eggs and maggots of different ſizes, and many cryſales among them; alſo, ſome neſts of black Ants, which appeared to be propagated in the ſame manner as I have before deſcribed reſpecting them. But in theſe black Ants' neſts, I found no eggs nor maggots, except ſome maggots employed in ſpinning their caſes, and ſome cryſales in the caſes or webs. I alſo here found a third ſort of Ants, of a bright yellow colour; theſe were as ſmall as the black Ants, and in them I could not diſcover any ſting. The maggots that came out of the eggs of theſe Ants, when brought to their full growth, changed into Ants without ſpinning a web, in like manner as I have ſaid of the red Ants.

For my ſtill farther ſatisfaction, I took out of my garden two ſeveral Ants' neſts, and the earth which contained the moſt Ants, with their eggs, maggots, and cryſales mixed ; theſe I put into a glaſs about nine inches deep, and eight inches in circumference, on which I put a glaſs cover, and gave the Ants for food ſome ſugar, a piece of a pear, and ſome cherries; placing the glaſs upon my deſk, in order, if poſſible, to diſcover the manner in which the Ants fed their maggots; but theſe the Ants carried away, and concealed under the earth in the beſt manner they were able, ſo that I could get a ſight of but few of them at a time. But, upon obſerving how motionleſs the maggots lay, I ſaw, that though ſhaken about or carried by the Ants from place to place, or whether lying in heaps or ſingly, and no Ants near them, they did not ſtir themſelves in the leaſt.

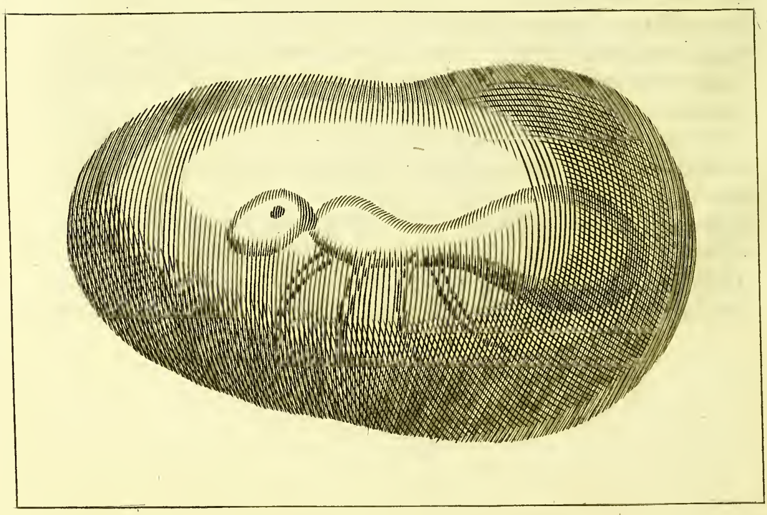
And as I frequently ſaw an Ant with its mouth cloſe to the head or mouth of a maggot, and in that poſture lie motionleſs for a time, I conſidered that probably one or more of the organs which the Ant has in its mouth, might be formed for no other end than to

put into the mouth of the maggots the food provided for them by the parents, which is what in beafts we name fuckling.

From thefe my obfervations, I concluded that probably the food which we fee the Ants in thefe countries carry to their nefts, is what they, for the time, have more than they can take into their ftomachs, and that they hide it in holes and corners in their nefts, until they have fed their young with the nutriment prepared in their ftomachs, and this being done, they refort to the provifions they have brought; which being prepared in their bodies to become nourifhment for their young, they can immediately, without quitting their nefts, fupply the young maggots with more food.

Addition, by the Translator.

Mr. Leeuwenhoek having in this Effay noticed particularly, the erroneous figure given by a German author, of what he calls the Ant's egg; that figure taken from Mr. Leeuwenhoek's copy of it, is here fubjoined: the Tranflator not chufing to infert it in the Folio Plate, left it fhould, at firft fight, be miftaken for a drawing given by our Author, with the accuracy of whofe figures this is altogether incongruous.



*Of a certain poiſonous Reptile, common in the Eaſt Indies, called in Latin, Millepeda Indica, in Dutch Duyſent-been, and in English vulgarly named the thouſand legs.**

I HAVE often heard mention made of the poiſonous puncture or bite of a certain noxious animal in the Eaſt Indies, called a Millepeda, or thouſand legs; this animal, as I have been told, creeps over the bodies of perſons aſleep, and being of a very cold nature, the perſon is induced by the unuſual ſenſation to move; if the perſon does not ſtir, the animal paſſes away without doing him any injury, but if he moves, it bites him with the two fangs or teeth, placed in the fore part of its head; and although no blood follows the bite, but only a very ſmall red or livid ſpot appears, yet an intolerable pain, with a ſwelling, enſues, which remains longer with ſome perſons than others; and, to relieve this pain, the moſt approved remedy is, to drown the animal in oil of Olives, and to anoint the wounded part with the oil.

Being deſirous to ſee one of theſe creatures, I gave in charge to ſome labourers employed in this town to unload the Eaſt India ſhips, to bring to me a living Millepeda, in order that I might if poſſible diſcover from what cauſe the poiſon in its bite proceeded; and thereupon they brought me one, about the ſize of my little finger, (though ſome of theſe are two or three fingers' length). I immediately took hold of it with a ſmall pair of pincers, near one of its two fangs or piercers, and examined the fang by the microſcope, and all the while, the animal was continually opening and ſhuttings its fangs, as endeavouring to bite. I ſaw that in each of

* We have in England an animal probably of this ſpecies, though not venomous, and of much ſmaller dimenſions, to which the name of thouſand legs is given, particularly by children. It is often found under ſtones or rubbiſh; it is ſeldom above two or three inches long, but in ſhape exactly ſimilar to the Millepeda here deſcribed; and wonderfully nimble in its motions.

theſe fangs, there was a round hole placed in a kind of groove or furrow leading towards the point, the intent of ſuch furrow being, as I conceive, to convey ſome kind of liquid iſſuing from the hole to the very point of the fang, while it is within the part wounded; and hence, I imagine, that the Millepeda by its bite, wounds and tears open ſome of the blood veſſels, or other veſſels within the ſkin, and at the ſame time infuſes a liquid into the wounded part, which liquid, I ſuppoſe, contains ſome noxious and very pungent ſalt, and that the pain felt is not occaſioned by the mere bite, but from that noxious or pungent liquid. I did intend to have purſued my obſervations ſtill farther, and had deſired the labourers to bring me more of thoſe animals, but none were then to be had; for though ſeveral were ſeen in the ſhip while the goods were unloading, they had been all killed upon the ſpot.

The forceps, fang, or what may be called the ſting of this Millepeda, I preſerved, and cauſed a drawing to be made of it. *Fig.* 8, A B C D E F, repreſents a part of this forceps or ſting, as ſeen through the microſcope. At C is to be ſeen the furrow or channel, and alſo the hole in it, through which this noxious animal, in wounding a man, injects the poiſonous liquor.

After this, I was preſented by a certain gentleman who is fond of collecting all ſorts of foreign animals, with a large Indian Millepeda, of which I have alſo cauſed a drawing to be made, becauſe many perſons know nothing of this poiſonous creature: this is ſhewn at *fig.* 9, G H I K L, and in this figure, I K, are the two fangs or ſtings, one of which is exhibited in the former figure, as ſeen through the microſcope.

Of the FLEA.

BEING desirous to know how long time was requisite for the egg of a Flea to produce a perfect Flea, I sat about making experiments to ascertain that fact; but it was not till after many trials that I could make a regular series of successful observations.

In the month of July, I enclosed several Fleas in a glass, that they might lay their eggs; the worms or maggots hatched from their eggs, I nursed with all the care I was able, feeding them every day with flies, which I first killed; these they devoured with great avidity, and thereby were very speedily increased in size. My observations on this subject are as follows:

On the 6th of July the worm came out of the egg.

On the 17th of July the worm appeared all over white, from whence I concluded that it was near dying; I twice offered it fresh flies for food, but it would not eat, and appeared to me to be motionless, but viewing it with the microscope, I saw that it was employed in spinning round itself a web or covering.

The 21st of July, this worm was changed into an aurelia or chrysalis, which was of a transparent white.

The 25th of July, this chrysalis assumed somewhat of a red colour, which continually grew deeper and deeper.

The 30th of July in the morning, it was entirely red, and in the evening the Flea it contained was leaping about the glass.

Hereby we see that in the middle of the summer, in four days time after an egg is laid by the Flea, it produces a worm or maggot, which in eleven days attains to its full growth, in four days time it is changed into a chrysalis, and in nine days more it becomes a perfect Flea.

Now if we lay it down, that it is with these creatures, as with most small animals, which in like manner undergo a change, (at

leaſt as far as I know) that their eggs are formed in their bodies before they come out of their aurelia ſtate; it will follow, that Fleas immediately upon their coming forth into life out of their webs, can copulate, and in three or four days time lay eggs, ſo that in the ſpace of twenty-eight days, from a newly laid egg, a Flea may be produced. This being the caſe, it is no wonder that in the ſummer we at ſome times perceive no Fleas in our houſes, and at other times they appear in great numbers without being brought from elſewhere; and if we alſo ſuppoſe, as I am well aſſured is the caſe, that Fleas, after they have copulated and laid the eggs which they had within them, do then die, we may conclude that we are not tormented for a long time by one and the ſame Flea.

But whereas many flying inſects do, within a few ſucceſſive days, lay all their eggs, this is not the caſe with Fleas; for a Flea, which in one day or night laid 4, 5, 6, 7, 8, and even 12 eggs, being afterwards ſhut up in a glaſs laid no more, though upon opening it, I found in its body many eggs remaining, ſome of which were exceedingly ſmall, whence I concluded that a Flea being ſo ſhut up, its laying no more eggs was occaſioned only by want of food. To ſatisfy myſelf in this reſpect, I took ſeveral female Fleas, after they had laid their firſt parcel of eggs, and contrived to confine them on my hand, in order that they might thence get a ſupply of nouriſhment: one of theſe ſucked the blood with great avidity, ſtanding as it were upon its head, and lifting up its hinder and middle feet with a quivering motion in them, this Flea the following day laid two or three eggs, and two or three days ſucceſſively afterwards, I placed it again on my hand, that it might get a further ſupply of food, but in vain, for it did nothing but leap about and endeavour to eſcape. The like happened to me in ſeveral other trials I made, but if the Fleas would have continually fed on my arm or hand, I would have done my beſt to aſcertain what is the natural term of this creature's life.

On this occaſion I cannot but obſerve that we meet with many

men who ſet themſelves in oppoſition to all new diſcoveries, being riveted to old opinions; yet are themſelves too indolent to investigate the ſubject, and the rather, as theſe reſearches into Nature's works are often very troubleſome, and bring no gain or profit. I ſhould endeavour to confute ſuch opponents, did I not think it would be loſt labour; and, indeed, I know not who would bestow ſo much labour as I have done on that minute and deſpiſed creature the Flea.

During the ſpace of two months and upwards, I put into the hands of my maid-ſervant four or more glaſſes, with cork ſtoppers, and directed her to confine in them as many as ſhe could catch of the largeſt ſort of Fleas, which are always females, with a caution to handle them gently, that none of the veins or veſſels in their bodies might be injured; and theſe Fleas I frequently removed into freſh glaſſes, to preſerve the eggs they laid from being ſoiled with the excrements they voided, in which, by the way I have plainly ſeen a great quantity of ſaline particles. Now, if, as has been obſerved above, in twenty-four days a perfect Flea is produced from an egg, we ſee that in a ſingle ſummer, or rather from the month of March to November, Fleas may be propagated ſeven or eight times, and ſo often may we be infeſted by new generations of them.

Not content with the preceding obſervations, I at ſeveral times trained up or nurſed the worms from the Fleas' eggs, till they grew to their full ſize, and I found that an egg in three days time produced a worm or maggot, and the worms or maggots in twelve or thirteen days, or little more, came to their full ſize. In particular, about the middle of Auguſt, I had two maggots, one of which came out of its egg about two hours before the other: the firſt of theſe I glued to the point of a ſmall braſs pin, and placing it before the microſcope, I gave it into the limner's hand, directing him to make a drawing of it, as nearly as he was able, for the worm moved itſelf about violently.

Plate XII. *fig.* 10, A B C exhibit this worm or maggots as glued to the pin, and appears to the view as if it lay on its back. A is the head, on which are two prominent parts, like horns, out of which proceed two other ſharp weapons, ſo ſmall that the limner did not perceive them until they were pointed out to him : between theſe horns appear two ſhorter prominent parts, which are not placed on the head, but at the lower part of it, where the mouth is ſituated : theſe parts the maggot uſes in moving from place to place, and alſo thoſe parts at the hind part of its body, which are pictured at C.

After this drawing was made, the worm ſtruggled ſo violently, that it got looſe from the glue, and in fixing it again on the pin, I happened to touch its head with ſome of the glue, whereby it was much hurt, and after remaining fixed for ſix hours, it died, ſoon after which I perceived the moiſture of its body ſo much evaporated, that it was contracted into many wrinkles, and at the end of twenty-four hours it was ſo dried as to looſe all likeneſs of a maggot, whereas the other which was hatched two hours later, and had remained fixed before the microſcope only two hours leſs, was not only alive, but its body as fully diſtended as if it had been newly hatched.

After this worm had been thirty hours before the microſcope and was ſtill very lively, I thought I ſaw its body not ſo completely filled out, and conſequently that ſome of its moiſture was evaporated. I frequently afterwards inſpected it, and at the end of ſixty-four hours, I ſaw it ſtill in motion, but its body diminiſhed in ſize : this day the ſun ſhone very hot.

At the expiration of four days and nights this worm was ſtill in motion. I did not examine it again till ſixteen hours afterwards, and its body was then much contracted ; the following morning the moiſture of it was ſo evaporated, that it could not be known to be a worm.

This circumstance, namely, that in living creatures there is ſo

little wafte of moifture, I have obferved, not only in this maggot, but in many minute animals, and even in the eggs of fmall infects, (among which I reckon thofe of the flea); for if the fruitful and barren eggs are placed befide each other, we fhall fee the barren ones dry away, whereas the fruitful ones will retain their moisture, at leaft, fo far as to afford the maggot in the egg sufficient for its nourifhment.

But what fhall we fay, when we obferve that the evaporation of the moifture in fmall living creatures, or their eggs, is fo little, infomuch, that a Flea fhall remain wrapt up in the web or cafe it fpins for four entire months, and yet its moifture not evaporate fo far as to caufe its death; whereas the moifture exhales from the body of a dead flea in fo fhort a fpace of time. In a word, we cannot but wonder at thefe appearances, and here our reflections muft terminate.

Before this worm, I have laft mentioned, came out of its egg, I placed the egg before the microfcope, and caufed a drawing to be made of it, becaufe I could not only fee the worm alive in the egg, and how its body was placed, but I could alfo difcern through the fhell, many of the joints in its body.

Fig. 11. D E represents the egg of the Flea with the worm or maggot in it, and which egg had been laid but three days. *Fig.* 12, F G, is the egg from which the maggot had crept out, and in which may be feen the manner how it had broke open the fhell. These eggs of the Flea are no larger when viewed by the naked eye, than fmall grains of fand: and as objects do not appear of the fame fize to the eyes of every one, fo it was with the limner who made this drawing; for the breadth of this egg appeared to me twice the fize here reprefented, and the worm alfo in the same proportion. I could, however, if neceffary, have made drawings of thefe objects from microfcopes of greater magnifying powers.

After the preceding remarks, I fo far fucceeded in the treatment

of ſeveral worms or maggots, that I had two which acquired their compleat portions of nouriſhment, and began to ſpin their webs; but, becauſe the bottom of the glaſs wherein they lay was ſtopped with a ſmooth piece of cork, and that preſſed in ſo tight that they could not penetrate it, they could not completely cover themſelves with the web. For in like manner as ſilk worms are placed in a paper of a round tapering form, called a cup, in order that it may fix its web on every ſide, and be completely covered by it; ſo the worm produced from the Flea's egg, when come to its full ſize, endeavours to creep into cracks, holes, or corners, in order to incloſe itſelf on every ſide with its web.

At ſeveral times I contrived to prevent the maggots encloſing themſelves completely in their webs, in order that I might the more eaſily diſcern their alteration into a chryſalis or aurelia. But how often ſoever I viewed them after they had ceaſed eating, and their change approached, I could only perceive that they placed themſelves in the ſame poſition as they had lain in the egg. But examining them in the afternoon in this poſture, I found upon looking at them three hours afterwards, that one of them was changed into a chryſalis. Upon viewing another of them through the microſcope, I perceived a mite upon its body, where it remained ſome time, and another larger mite running about the glaſs, whereupon I concluded, that at the time thoſe worms are in their ſtate of change and unable to defend themſelves, the mites will ſeize and feed upon them. After this worm had thus laid for ſome hours, I ſaw that its body was somewhat contracted, whereupon placing it before the microſcope, I perceived three holes pierced in its body, part of its ſkin ſtripped off, and the body of it beginning to dry away.

Hereupon I began to conſider, that this web or covering ſpun by the Flea's worm or maggot is neceſſary to it, and that without it, thoſe creatures could not eaſily be propagated; for there are ſeldom any fragments of food, or the bodies of ſmall animals lying

about our houſes, but they are immediately found out by mites who come to feed upon them; and, in the preſent caſe, though I thought I had perfectly ſecured theſe maggots from the mites, becauſe the glaſs tube wherein I had put them, which was almoſt an inch in diameter, and five inches long, was ſo cloſely ſtopped at each end with cork, that I ſhould have thought it impoſſible for any mite to get in; yet now I found the contrary to be the caſe: and indeed, I have often found mites feeding on things in places where I wondered they could ſo ſoon diſcover their food. And now, upon ſeeing theſe mites about the maggots, it occurred to my thoughts, whether or no thoſe ſmall and very ſlender prominent parts in the maggot, which in *fig.* 10, are ſhewn at A, might not be weapons deſigned for its defence againſt the mite.

But the web ſpun by the maggot, and wherein it incloſes itſelf, before its transformation, protects it in that ſtate from all attacks of the mite; and the like we may conclude, to be the caſe with the ſilk worm, which while in its ſtate of tranſmutation into a butterfly, is protected by its cone or caſe, not from mites, but from birds; and this I am well aſſured is the caſe with all caterpillars, which when of full growth, and the time of their tranſmutation approaches, do incloſe themſelves in ſome kind of web, or covering.

The maggot which I have ſaid I ſaw changed into a chryſalis, I the next morning placed before a microſcope, and delivered it to the limner, to make a drawing of it.

Fig. 13, A B C D E F, is this chryſalis; A K, are the horns of the yet unformed Flea; L G B, is one of its fore legs, which riſe out of the head at L; and herein, at B, may be ſeen all the ſmall joints which will be found in the legs and feet when perfectly formed: the like joints may alſo be ſeen in C and D.

H C is one of the middle feet, and at G may be ſeen that the fore feet partly cover the middle feet, and theſe in part cover the hind feet; I D and C H are the two hind feet, which are in part covered by the middle ones.

Fig. 14, M N O P Q, reprefent the pellicle put off by the maggot, when it changed into a chryfalis, leaving the wrinkles in it as they here appear ; and at M are the places where the maggot's horns ftood.

Three days after this drawing had been made, I faw that the chryfalis was very much altered, and many parts appeared in it which till then could not at all be diftinguifhed ; therefore I caufed a drawing to be made of this alfo, to fhew fuch alteration.

Fig. 15, A S T V, is this drawing, taken as exactly as the limner was able to reprefent this fecond appearance of the chryfalis.

Fig. 16, A B C D E F G H I K L M N, reprefents an entire Flea, glued by the back to the point of a needle, and fo placed before the microfcope, but being firft killed, becaufe it would be impoffible to make a drawing of a Flea while alive. Nor, indeed, did I intend to have given a drawing of the whole Flea, but a certain learned gentleman ftrongly urged me to it, faying, that the world would thereby be more convinced, that Fleas are not produced from corruption, but in the ordinary way of generation, and I therefore complied with his request. In this animal a great number of blood vessels were to be seen, particularly in the belly, but the limner only drew a few of them, faying, that it was not in his power to imitate the remainder in his drawing.

This Flea appeared to me, through the fame microfcope, eight times larger than here fhewn, though the limner declared that it did not feem any larger to him. Nor could I ever have believed that there was fuch a diverfity in the fight of different people as I now find to be the cafe. But this limner was very fhort fighted.

At D E F are fhewn the hind feet ; F G H the middle feet ; and I K L the fore feet ; placed on the head : and between thefe lie the organs or weapons with which the Flea pierces or bites, and feeks its food. L M N are two horns on the Flea's head.

I had formerly figured to myfelf the manner in which Fleas must get their food out of the bodies of men and animals ; but becaufe

thoſe parts in a Flea which I heretofore took to be the piercers or weapons by which it draws the blood, appeared now to be the ſheath or caſe of the real piercers, which divide into two parts when the Flea thruſts out its piercers from between them. I will now deſcribe how theſe parts appeared to me upon the examination of three ſeveral Fleas.

Fig. 17, A B C are the two horns on the head, which have been ſhewn in the laſt figure, each of them has three joints, beſides the joint fixed to the head; D E is the ſheath or caſe, with the ſtings or piercers, which in a manner fit or lock into each other, as I obſerved while I was endeavouring to ſeparate them. F A D is part of the head.

Fig. 18, G H I K, repreſents alſo the piercers, and their caſe or ſheath, placed in the head, between the two fore feet; which piercers and their ſheath are very difficultly diſcovered, unleſs the fore feet be firſt cut off cloſe to the head, and even thus they will ſeldom be ſeen; becauſe the Flea, when not uſing its piercers, draws them within the ſheath, and places them cloſe to its body. But as a Flea, though its fore feet are cut off, will yet live a long time, my way always was, when I endeavoured to get a view of the piercers, to cut off its head, leſt, when I had removed the piercers from their poſition cloſe to the body, and ſeparated them from each other, the Flea ſhould draw them again out of ſight.

G I, and G K, are two parts, which, having cavities in them, oppoſite to each other, conſtitute the ſheath of the piercers; which piercers are placed in the cavity marked at G H. Theſe two ſtings or piercers might indeed be taken for a ſingle one; but if we view the figure narrowly, it will appear that the limner has pictured the ſecond ſting as it were within the firſt, and not reaching quite to the end at H.

Fig. 19, L M N O P, exhibits the ſheath of the ſtings or piercers as I found them in a third Flea. Here I had better ſucceſs than in the diſſection of the two former Fleas. For at L N and L O are

reprefented thofe two parts which, when joined together as I have before faid, conftitute the fheath, and are covered with many hairs. At L M and L P are the ftings or piercers; and in one of them, L M, may be feen the cavity in which the other of them, L P, is placed, when both are at reft: and when thefe two piercers are inclofed in the fheath, thefe four organs may be taken for one fingle fting.

While I was employed about thefe obfervations, my fervants brought me a Flea, fixed to a fmall needle. Upon placing this before the microfcope, I faw not much motion in its feet, and the ftings or piercers were put forth from their cafe; and the fting, L P, which was inclofed in the cavity of the fting, L M, was moved up and down in a very pretty manner, with great fwiftnefs, and as it were in play.

Moreover, farther to gratify the curious, I pulled off one of the Flea's hind legs; in doing which, part of the mufcle belonging to it was feparated from the body, this I fixed to the point of a brafs pin, in order the better to place the whole leg before a microfcope of greater magnifying power, and to caufe a drawing to be made of its true fhape.

Fig. 20, B C D E F G H I K L M N, reprefents this hind leg; A B is the mufcle, feparated from the animal's body at A. The firft joint is fhewn at C D M N, and B C N is the part by which it had been united to the body: in this firft joint might very plainly be feen four diftinct parts, lying longitudinally, and probably were tendons and blood veffels; which veffels, and their more minute ramifications, I could plainly fee. This joint, and all the others feen through the microfcope, exhibited a tranfparent yellow colour, like that of amber.

Between the letters D E L M may be feen the perfect articulation whereby this firft was united to the fecond joint.

E F K L, reprefents the fecond joint, in the middle of which not only the nervous or mufcular parts lying lengthways, were plainly

to be ſeen, but alſo a blood veſſel, diſtinctly appearing to be compoſed of annular parts, in which manner the blood veſſels of Fleas and other inſects are formed, ſimilar to the aſpera arteria, or the wind pipe in the lungs of animals. And at one time, having a Flea fixed by its hind part to the point of a needle, before the microſcope, I ſaw throughout the whole length, on each ſide of this joint, as between E F and L K, and alſo, in the middle of it, a wonderfully ſwift motion or current of the juices, which appeared to me the plainer as there were globules mixed with thoſe juices: but I did not notice this motion, except at the time the Flea was moving its leg or foot, and then this motion continued for ſome time after the leg and foot were at reſt. Many perſons viewing this would ſay, that they had ſeen the circulation of the blood in the Flea's foot.

Moreover, upon the motion of the leg, as well in this joint as in the others, there was ſo great an agitation of the component parts as is inconceivable. This agitation, I concluded, was only cauſed in the fleshy muſcles of the leg, becauſe it partly continued after the death of the Flea; but it was impoſſible for the limner to imitate, in his drawing, all the parts he ſaw: and he often, while making the drawing of this leg and the other parts of the animal's body, broke out into an exclamation, " Heavens! what wonders here are in ſo ſmall a creature!"

F G I K is the third joint of the leg, in which may be ſeen many ſharp-pointed particles, ſticking out like thorns.

At G H I are ſhewn the five laſt or ſmalleſt joints of the Flea's leg, all formed in the ſame manner; and here plainly appears how all theſe joints mutually depend upon, and are ſubordinate to, each other.

At H are two perfectly formed claws: with theſe every one of the feet is furniſhed, and the Flea uſes them with ſuch wonderful ſwiftneſs and dexterity, that though when ſhut up in the glaſs he leaps about, and touches only a ſmall particle of dirt or impurity,

though it were only a particle of his own excrement adhering to the glaſs, he can, by a touch only of one claw, cling to it.

Now if we reflect on this wonderful and complicated formation of joints in a Flea's leg, we ſhall ceaſe wondering that it can leap to ſo great a height as we ſee; nor aſk the queſtion (which I have often had put to me) whether Fleas had wings to carry them ſo far and high?

Farther, I cut a maggot, produced from the Flea's egg, and was almoſt full grown, into two pieces, and placed each part before a microſcope of the ſame magnifying power as that by which the maggot newly hatched was drawn: but if I had placed the maggot entire before the microſcope it would have been impoſſible, by reaſon of its continual motion, for the limner to have given a true figure of it.

Fig. 21. A B C D, repreſents the head and three firſt joints of this maggot's body: I had placed it upright before the microſcope, but before the limner had got it, the head was inclined downwards, as ſhewn at A B C; nevertheleſs this piece of a maggot, for the ſpace of an hour, moved itſelf, and often lifted up the head.

Fig. 22. E F G repreſents the four laſt joints of the maggot, conſtituting its hind part, in which not only may plainly be ſeen, at F, the organs or limbs with which the tail of this maggot is provided, but the place may alſo be diſtinguiſhed where the excrements are voided.

Now this maggot not being provided with any limbs or organs for moving from place to place, except what are ſeen in *fig.* 21, at D, in its head; and in *fig.* 22, as F. in its tail: the many hairs it has on its body are a great help to its motion; but, at the ſame time, are a very great hindrance to it, if it meets with any liquid matter. So that even the hundredth part of the liquid excrement voided by the Flea at one time, is ſufficient not only to adhere to thoſe hairs on the maggot, ſo as to ſtop its progreſs, but alſo to kill the maggot itſelf, by reaſon that it is not able to extricate itſelf from the moiſture: therefore, as we ſee how little able theſe mag-

gots are to endure moiſture, we may conclude that, if in places where Fleas abound, the floors or pavements be well wetted with water, the maggots may be deſtroyed, and conſequently the places cleared of Fleas.

I have cauſed a drawing to be made of the glaſs wherein I incloſed the Fleas I had under my obſervation, for the information of any who may chuſe to proſecute my experiments. *Fig.* 23, N O P Q, is this glaſs, the cavity or orifice of which, N O Q, is about the fourth part of an inch in diameter: the ſphere or globular part, O P Q, is ſomething larger, according as may happen in the blowing it. I did not put more than two or three Fleas at a time into one of theſe glaſſes, and as ſoon as they had laid any eggs, I took them out of the glaſs, that it might not be ſoiled by their excrements or the vapour of their bodies, otherwiſe the hairs on the bodies of the maggots would ſtick to thoſe foulneſſes, ſo that they could not be taken out of the glaſs, but would there expire.

I have now for ſeveral years obſerved Fleas to lay eggs, and from ſuch of the eggs as were fruitful, maggots produced, which maggots were afterwards changed into Fleas as before deſcribed

Since we then ſee ſo plainly, that the Flea is endowed with as great perfection in its kind, as any large animal, all whoſe limbs may be ſeen with the naked eye, can any one give credit to the idle tales of old ? one aſſerting that Fleas are produced from ſand ; another, from duſt ; and another from the dung of pigeons ; and laſtly, from urine : for that Fleas can be produced from duſt and filth, I utterly deny, as appearing to me impoſſible : nevertheleſs, that out of the duſt of a floor or pavement, Fleas will ſometimes come forth, I readily grant ; but this is brought about in quite a different manner.

If many children be kept in a room, in which the Fleas are not every day extirpated, and the floor or pavement is ſometimes ſwept ; or if a floor or pavement be ſwept near to a place where a dog is accuſtomed to lye, there may very eaſily be taken up among

the ſweepings, not only young maggots from the eggs of Fleas, but full grown ones, and out of the chinks or cracks maggots may be ſwept, ſome of which are ſpinning their webs, and others, which have completed them, and are changed into cryſales; ſo that out of ſuch ſweepings Fleas may come forth day after day: and it may happen, that maggots newly hatched, may among ſuch duſt and rubbiſh find ſomething to afford them nouriſhment, ſo as to grow up to be complete Fleas. And I am the more inclined to believe that this may be the caſe, becauſe I could never get out of any dog, how much ſoever he was infeſted with Fleas, any of their eggs, nor ſee any about his body. But if we take a cuſhion covered with a green or any other dark colour, and let a dog lie on it who is infested with Fleas (which I have often done), we ſhall find that the Fleas do not lay their eggs upon or near the dog, but the eggs will be found in the corners and ſewings of the cuſhion, where, by their whiteneſs, they will eaſily be diſcerned.

We have, indeed, moderns who favour those old opinions, of whom I will only ſay, that if they were provided with a good microſcope, and would attentively beſtow a few days, as I have done many, in the inveſtigation of the ſubject, they would not broach ſuch fables and childiſh tales, as they now write and publiſh to the world.

On the ſeeds of Trees, with the author's reaſoning and obſervations on the poſſibility of intermixing two different ſpecies of tree, ſo as to produce a third, partaking of the nature of both. A ſingular peculiarity in the ſeed of Cotton.

UPON an accurate examination of every kind of ſeed, we ſhall diſcover in it the origin or firſt formation of the leaves and root of the future tree or plant, according to the reſpective ſpecies of ſuch ſeed.

This is particularly obſervable in the ſeed of the Aſh, which is repreſented of its natural ſize in plate XIII. *fig.* 1. In this ſeed, though no bigger than here pictured, I diſcovered not only two leaves, but also that part from which the root would grow, being all very large in proportion to the ſize of the ſeed.

Again, all ſeeds in their formation, on the parent plant, receive their nouriſhment from a ſmall ſtalk, ſtring, or ligament, conſiſting of many veſſels through which the nutritive ſubſtance is conveyed, analogous to that which in animals is called the navel ſtring. In ſome ſeeds this ſtalk or ligament is very ſhort, in others as long, or longer than the ſeed itſelf. I will give ſome inſtances of this, beginning with the ſeed of the Aſh.

Fig. 2. A B is the half of the ſhell, caſe, or covering, incloſing a ſeed of the Aſh. A D ſhews where the ſmall ſeed E F was placed, and from whence being taken out, as repreſented in the figure, there appears the ſtring or ligament A F, through which the whole ſeed E F received its nouriſhment, being derived from the part A, which was joined to the tree, and through which alone the ſame can be conveyed. It is also to be obſerved, that the part in the ſeed whence the future root will proceed, is at the point F, where the ligament is joined to it; ſo that the leaves of the future tree when in the ſeed, while it is united to the parent tree, are placed,

as I may ſay, with their points downwards, and their roots and ſtalks upwards. But we muſt conſider that the ſeeds of the Aſh, which grow in * cluſters on the tree, do at length, by their weight, hang downwards, and that the ſame is the caſe with apples and pears, which while they are very ſmall, ſtand erect on their ſtalks, and conſequently the origin of the future plants formed in them, is placed with its root upwards; but when the fruit grows to ſuch a ſize, as by its gravity to hang downwards, then the young plant in the ſeed or kernel, has its root and leaves in the ſame poſition as it will have when growing.

Upon opening the ſeed of the Aſh, I found in the middle of it two large leaves, and alſo the firſt rudiment, or beginning of that part which would become a root, and theſe leaves were larger than I have obſerved in the ſeed of any plant whatever. Theſe leaves when examined by the microſcope, appeared as in *fig.* 3. C D E F, and I found them to conſiſt of an immenſe number of thin round prominent globules, which I endeavoured to imitate in the drawing, though they are not there repreſented nearly ſo ſmall as they appeared to me. I alſo ſaw in thoſe leaves a great number of fibres, or more properly veſſels covered with wonderfully minute globules; which veſſels, but without the globules, are repreſented in the figure: theſe veſſels ariſe from the inſide of that part whence the future ſtem or trunk of the tree, and alſo the root are produced; which is pointed out in the figure at A B C F G H. This laſt mentioned part which would produce the ſtem and root, beſides thoſe veſſels from which the fibres of the leaves iſſue, and which may properly be conſidered as the marrow or ſap of the wood, is moreover provided with many veſſels; and in order to repreſent thoſe veſſels in a clearer light, I cut a piece tranſverſely, at the place marked with the letters B G, and having placed the piece or ſlice before the microſcope, I made as exact a drawing as I was able of it, with all the veſſels in it, as

* In England, theſe cluſters of ſeeds on the Aſh, from the reſemblance they bear to a bunch of keys, are called Aſh-keys.

they appeared to me, a reprefentation of which is given at *fig.* 4. I K L M. In this fmall particle, I not only judged that there were about a thoufand veffels taking their courfe upwards, but it alfo gave me a reprefentation of a complete branch of the tree cut tranfverfely; for the external furface reprefented the bark; the part next it, in which are very fmall dark fpots, denoted the wood; which dark colour was only caufed by this, that in cutting the flice, the knife had fqueezed together, or ftopped, the fmall tubes or veffels of which it was compofed; the middle part, the fap, or thofe veffels which would produce the ftem or branch, and being cut tranfverfely, appeared round bodies. But this figure was drawn from a microfcope of greater magnifying power than that ufed in the former, in order to fhew the nature of this ftem and root more diftinctly, fo that the diameter of this figure at I L, is about twice the diameter of the part marked in the laft figure by B G.

Moreover, in this fmall trunk or ftem, I perceived in fome places, fome round particles, as if they were fmall round corpufcles in the tubes or veffels, which are reprefented between the letters A and B.

I have thought it right here to exhibit the fize of the young leaves in the feeds of fome other trees, to fhew that it by no means follows of confequence, that the largeft feeds contain the largeft rudiments, or originals, of their future plants, for the young plants in the fmall feeds of the Afh are vaftly fuperior in fize to the very fmall leaves which the large feed of the Walnut contains, and which, as feen by the microfcope, are fhewn at *fig.* 5 and 6, the originals of which I took from two feeds of that fpecies, in order to note if there might be any material difference between any two of them. In thefe figures I have only reprefented the leaves, becaufe in the rudiments or originals of the future ftems and roots, (which in this feed are fomewhat longer than the leaves), I perceived little or no difference in the form from thofe in other feeds. In drawing thefe two figures, I found it quite impoffible to repre-

ſent the very minute globules of which theſe young and unformed leaves were compoſed.

I have in another place ſaid, that there are no blood veſſels intermixed among the ſmall fibres or fleſhy ligaments of which a fleſhy fibre conſiſts; but that the blood veſſels which ſurround the fleſhy fibres, are placed in or between membranes; and I have alſo laid down my idea, how far the fleſhy fibres are nouriſhed from the blood veſſels; ſo in this caſe, I ſay, that all the leaves of trees and plants are formed of globules (beſides the veſſels or fibres of the leaves), all incloſed in a membrane conſtituting the ſurface of the leaf, and the manner in which, I imagine, that theſe globules, although they touch no veſſels, are yet nouriſhed by the veſſels, I take to be this: we muſt first underſtand, that almoſt all the leaves of trees and plants, while united to the plant, conſiſt of two third parts water or a watery ſubſtance, and that the globules, of which for the greateſt part, the leaves conſiſt, do not lie ſingly or ſeparate, but many of them heaped together. Thus let us ſuppoſe *fig.* 7, A B C D E, to be the veſſels in a ſmall piece of a leaf, and that theſe veſſels have a great quantity of globules, which in the nouriſhment of the leaf are ſupplied and ſupported by the veſſels: then upon the globule F receiving a ſupply of nouriſhment, particularly water, from the veſſels to which it is cloſely conjoined, it muſt neceſſarily impart this nouriſhment to G, and that the ſame nutritive ſubſtance will be conveyed from G to H, not only becauſe theſe globules are cloſely conjoined together, but alſo becauſe as before mentioned, the greateſt part of the ſubſtance of the leaf, and conſequently of each globule, conſiſts of a watery matter or ſubſtance; and thus, thoſe globules which are next the veſſels, cannot receive any nouriſhment from them, but they muſt impart of it to thoſe globules next to them, and thoſe again to others. I have formerly compared this communication of nouriſhment to dry globules or balls of clay, which we will ſuppoſe lying in a glaſs, and only one of thoſe balls to be made wet, and the moiſtening being continued,

the other balls of clay contiguous to this moift one, receive the moifture from it and become wet, and thefe again lying in contact with others, at length all the globules or balls of clay will, by means of the firft globule, become wet; and this is the cafe in the nourifhment of leaves and alfo of fruits.

Fig. 8, A B C reprefents the outfide of the young leaves in an Almond, and upon removing them, I perceive feveral leaves inclofed in them. *Fig* 9, A B C, is the outfide of the leaves in the kernel of a Cherry, and *fig.* 10, A B C, the leaves, as they appeared to me, in the kernel of an Apple.

Now, if in fuch fmall feeds as thofe which produce the Afh (one of the largeft trees growing in this country), and which feed is fo light, that fix of them fcarcely weigh four grains, if in thefe feeds, I fay, we difcover, not only the perfect leaves with their veffels, but alfo the ftem and root of the future tree, and thefe much plainer and larger than in the Walnut or Hazle, we fhall alfo eafily perceive that wife and provident Nature in all her operations, and efpecially in the propagation of plants and animals, perfects her work, by ways and means fimilar or analogous. * For the feeds of trees and plants not only contain in them the origin of the future plant, but alfo a white fubftance which we denominate meal, in order to nourifh and fupport the young plant, until it has ftruck its roots into the earth and can draw nourifhment from thence. Befides which, many feeds are provided with an oily fubftance, whereby the young leaves and plants are kept from drying up, and † many feeds which have not this oil will not remain long good out of the ground.

* In the original it is "all feeds muft neceffarily be provided with a mealy fubftance," &c. but this is a miftake which the author corrects prefently afterwards in his account of the feed of Cotton.

† This is particularly the cafe with the Acorn, which by nature is defigned to vegetate immediately after it quits the tree.

Fig. 11. G H I is the half of the hard ſhell of the Filbert. K L M is the nut or kernel taken out of the ſhell. G is that part of the ſhell which was united to the tree, in which part there is a very looſe or ſpongy paſſage through which the ligament of the nut or kernel paſſes, as G L, to convey the nutriment for the formation of the young plant, and alſo a ſufficient quantity of aliment to ſupport it, when the nut is ſown or planted, until the root of that young plant in the nut, has extended itſelf ſo far out of the hard ſhell as to reach the earth. This figure alſo ſhews that the ſubſtance, which through the ligament is conveyed for the ſupport of the nut, does not at all ariſe from the hard ſhell, but that the ligament having a paſſage through the ſhell, conveys the nouriſhment from the tree itſelf.

Theſe ſtrings or ligaments, whereby the young plant and the reſt of the ſubſtance of ſeeds is nouriſhed, have alſo coats or barks of their own, and within this coat or bark in the ligament of a Filbert, I think there are above an hundred ſmall veſſels, all which, as far as I could ſee, are formed of fibres in a twiſted ſhape, in like manner as if one were to wind a piece of ſmall copper wire round a pin, and then draw out the pin from the wire, leaving it in the form delineated at *fig.* 12, A and B.

It is worthy of conſideration reſpecting theſe ligaments, that in almoſt all ſeeds the ligament is joined to that part of the nut or kernel, from which the young plant will ſhoot, as may be ſeen at *fig.* 13, letter A, (which is a Filbert drawn ſomewhat larger than the natural ſize, in order to ſhew more eaſily the courſe of the veſſels proceeding from the ligament), in which figure the ligament takes its courſe from A to B, and in its paſſage ſpreads itſelf into divers branches, and theſe again into ſmaller ones; and thus ſpread through the whole nut, which ligament, or the veſſels ariſing from it tend to the ſame point, and finiſh where they begun, that is, where the young plant will be produced.

I made a tranſverſe ſection of the ligament by which the Almond is nouriſhed, becauſe it is ſomewhat thicker than that in the Filbert,

and made a drawing of it as ſeen through the microſcope, which is to be seen at *fig.* 14, C D E F G. This ligament is divided into ſeven compartments, which compartments are of a reddiſh colour; the veſſels they contain, I have repreſented in the figure as they are placed in one of the compartments at F G H; and from a view of the veſſels in this ſingle compartment, any one may eaſily figure to himſelf the great number of veſſels in the ligament, by which the Almond, the Filbert, and moſt ſeeds are nouriſhed; for upon examining the ligament of the Filbert, I found no difference in it, except that the ligament of the Almond was larger in every part.

In my remarks and contemplations reſpecting the propagation of trees and the nature of ſeeds, I turned my attention to the Willow, which is no otherwiſe propagated among us, than by cutting off a branch and planting it in the ground, where it grows to a tree. But becauſe I had obſerved ſeveral Willows growing in fields and on the banks of ſtreams, in places where I judged they were not planted by hand in the way I have deſcribed, but to have been produced from ſeeds; I turned my mind to diſcover what was the fruit or produce of the Willow, in order to diſcover the nature of its ſeed. The only fruit of the Willow is a kind of wool or cotton produced on it, about the beginning of the month of June, and at that ſeaſon, upon examining this cotton, I ſaw lying in it many dark coloured particles, a little larger than grains of ſand. Upon viewing theſe particles by the microſcope, I found them to be the ſeed of the tree, and that the cotton and theſe ſeeds were formed in a kind of cells of a violet colour, and of theſe cells I counted ſeventy-five, placed near each other on a ſmall branch, which ſeemed deſtined for no other purpoſe but to produce the ſeed, and in each of theſe cells, three, four, or five ſmall ſeeds lying among the cotton; and I could perceive that the cotton was formed out of the ſeeds in the time of their growth. Theſe ſeeds were of the ſize repreſented at *fig.* 15: the cotton of the ſeed was in two, three, four, five, and ſometimes of ſix filaments, joined together by a kind of knot and united to the

ſeed: and when the ſeed and cotton were fully ripe, the cells in which they were contained, burſt open, and the cotton looſed itſelf from the ſeed, and then each particle of cotton which before had laid in regular order beſide each other, as at *fig.* 16, ſtarted aſunder, as repreſented at *fig.* 17; by which means a ſmall quantity of the filaments of the cotton being ſo widely diſperſed, was, with the leaſt wind carried away, bearing with it many of thoſe minute ſeeds, and flying over walls and buildings, could depoſit the ſeeds of the Willow in very diſtant grounds. Upon narrowly inſpecting this ſeed, I ſaw that the part from whence the future root would ariſe, and which was about one third of the ſeed, was provided with many veſſels which ſeemed to conſiſt, for the moſt part, of oblong and round particles. The reſt of the ſeed conſiſted of two parts joined cloſe together like leaves, very ſolid in proportion to their ſize, of a dark green colour, and ſeeming to be formed of globules. Having ſeparated theſe aſunder, I ſaw two very minute protuberances, which I imagined were the beginnings of two leaves, and the origin of the future tree; hence I concluded, that the firſt two parts which I have compared to leaves, were only deſigned to afford nouriſhment to the young plant, until it ſhould be provided with roots, by which it could extract its nouriſhment from the earth.

In order further to ſatisfy my curioſity reſpecting this very minute ſeed, from which ſo large a tree as the Willow is produced, I took ſome of the ſeeds, and, in the month of June, placed them in moiſt ſand in my cloſet, in order to ſee how the beginning of vegetation would be performed in this very ſmall ſeed. But before I put the ſeed into the ſand, and while it was yet very dry, I viewed it with the ſame microſcope from which the drawings of the preceding young formed plants are taken, and it appeared as repreſented at *fig.* 18, A B C D E F: but theſe ſeeds were of very different ſhapes, for in drying they became crooked, ſome more than others. A B E F, is that part of the ſeed from whence the root would ariſe.

When this ſeed had lain in the wet ſand thirty-ſix hours, it appeared as at *fig.* 19, G H I K L. And here it appears how much of the ſeed was to form the root, which is marked by the letters G H K L ; and it was not only grown longer, but in the very ſhort ſpace I have mentioned, ſix diſtinct roots had grown out, as repreſented at G L ; and thoſe parts, H I K, which I have deſcribed to be like leaves, and which, while the ſeed was dry, could not be ſeparated without the greateſt difficulty, now opened as it were of themſelves, to make room for the plant contained between them. And when this ſeed had remained in the moiſt ſand ſeventy-two hours, I found that the roots had ſpread themſelves into divers branches, which were ſo ſtrongly twiſted among the particles of ſand, that it was impoſſible to ſeparate them without breaking the roots.

It is well worthy of remark, reſpecting the Willow, that the ſeeds are ripe before the leaves on the tree are grown to their proper ſize, whereas the fruits of moſt trees, and conſequently the ſeeds contained in them, do not arrive at maturity till much later in the ſummer, or elſe in autumn ; ſo that in the Willow, the ſeed being ripe in the ſpring, a new tree may be produced from it the ſame year. This I have alſo obſerved in the Elm : for, about the end of May, I took ſome ſeeds, which are of a very ſmall ſize, from an Elm, when the leaves on the tree were about half grown : theſe ſeeds, which were dry, I put into wet ſand, and after three days they began to grow. I have alſo found that the Poplar tree, which produces a cotton formed with two flat ſides, like the Willow cotton and the Indian cotton, produces its ſeed about the end of May, or the beginning of June.

Here we ſee, that in ſuch a ſmall ſeed as that of the Willow, not only the young plant and the root of it, which is provided with veſſels as if it was a complete tree, can be ſeen, but alſo that within ſix-and-thirty hours the ſeed will begin to grow, even in a cloſe room, ſo that the young roots may be diſtinctly ſeen.

I have ſometimes thought, that if it was poſſible to take out the young plant from any ſeed, and unite it to another ſeed, in that place where the young plant was formed in that ſeed, a tree or fruit would from thence be produced of a ſpecies unknown before: as, for example, if we could take out the young plant from the Walnut, and unite it to the ſubſtance of a Cheſnut, a tree would be produced not like either of the trees from whence thoſe fruits were reſpectively produced, and conſequently a tree of a new ſpecies. And a certain eminent gentleman hearing me mention this, urged me ſtrongly to make the experiment, ſaying, that though of thoſe two ſeeds only one could vegetate, yet we might expect that ſome third plant of a new ſpecies would be the reſult. For my part, I conſidered the thing as impoſſible to be performed, and ſo I found it upon further inveſtigation of the ſubject; but as there are not many men well informed in the firſt formation or growth of trees and plants, I will here deſcribe the nature of the firſt formation of the young plant, both in a Cheſnut and a Walnut.

Fig. 20 is a Cheſnut, broken in half; A is the rudiment or firſt beginning of the plant, and though the Cheſnut is a very large ſeed, yet the original or firſt formed leaves can very rarely be diſcovered in it; and indeed I muſt confeſs, that among many Cheſnuts I found but one in which I could diſcover the two leaves of the young plant; the upper part of the young plant was ſomewhat of a round form, and furniſhed with a ſort of cavity. This beginning of the plant, marked at A, is not only firmly united to the ſubſtance of the nut, but there is one remarkable particular in it, different from other plants, that it has not only two ſtrings or ligaments to convey nouriſhment from the Cheſnut to the young plant, until it is of ſize and ſtrength to draw nouriſhment from the earth, but theſe ligaments are provided with a great number of veſſels, having their riſe in the Cheſnut, to convey the nutritious juices; and theſe veſſels produce others in the root and ſtem. Theſe

veſſels are of the ſame formation as I have already deſcribed the ligaments of the Almond and Filbert.

Fig. 21, A B C is a Cheſnut, which I put into an earthen veſſel filled with moiſt ſand, and placed in the chimney, it being in the winter ſeaſon, and watered it every ſecond, third, or fourth day, according as the difference of heat ſeemed to require: C D and A E, are the two ligaments to which the young plant is united; F G, is that part which will ſpring up to a ſtem; G, indicates the leaves beginning to ſhoot; and E H D, the root. And here we ſee to what uſe the Cheſnut is deſtined, namely, to ſupport and nouriſh that ſmall part contained in it, which is to conſtitute the future plant; and this is performed through the two ligaments, until the young plant and its root are grown to a ſufficient ſize to draw their nouriſhment from the earth.

Fig. 22, is a Walnut, ſo divided or cut as to ſhew, at I K, the origin or firſt beginning of the young plant. And whereas I have ſaid, that in a Cheſnut the young leaves are not to be diſtinguiſhed; on the contrary, they may eaſily be ſeen in a Walnut: for in every one of them that I examined, I could, with the microſcope, as plainly ſee the leaves, as with the naked eye we can ſee young leaves in the ſpring. I, denotes that part which will grow into branches and a tree; K, the ſharp-pointed part from which the root will iſſue.

I alſo treated ſeveral Walnuts in the ſame manner as I have mentioned reſpecting Cheſnuts, in order to obſerve their vegetation. *Fig.* 23, L M N O P, is the Walnut; L and P, are the ligaments, to convey nouriſhment from the root to the young plant. Theſe are provided with a great number of veſſels, which, from the obſervations I made on two ſeveral ſeeds, I was well aſſured were ſpread through the whole ſubſtance of the nut, or ſeed, in order to convey its moſt nutritious juices to the young plant, until it ſhould be ſufficiently grown to draw its nouriſhment from the earth; R P, is the root.

From theſe obſervations, ſhewing the cloſe connection of the young plant by the ligaments with their multitude of veſſels, with that mealy ſubſtance which we call the ſeed, it plainly appears, that we cannot take out the young plant from ſuch ſeeds, without breaking thoſe ligaments and their veſſels; and when they are broken the young plant is dead, and cannot be removed into any other ſeed; ſo that it ſeems to me impoſſible to remove the young plant from a Cheſnut to a Walnut, and ſo to place it that the Cheſnut ſhall grow in the Walnut.

And though we may be able to take out the young plant from the ſeeds of Aſh, Lime-tree, Gooſeberries, Currants, or the like, without breaking the ligaments, or rather without obſerving them, yet we muſt conſider that were there are no ligaments with veſſels in them, yet, in ſtead of them, the globules compoſing the mealy ſubſtance of the ſeed are placed in ſuch order, and ſo cloſely united to the beginning young plant, that they either ſupply the place of veſſels, or in reality are veſſels, the true ſtructure of which is to us inſcrutable. Moreover, there will be always a conſiderable difference between the ſize of the young plants in different ſeeds, and conſequently the place whence the plant is taken in one ſeed, will be too ſmall or too large to receive the young plant from another. Add to which, that we cannot take out the plant without breaking the ſeed, which, by ſuch breaking, will become uſeleſs. So that it is plain there is no poſſibility of taking the young plant out of one ſeed, and uniting it with the farina or mealy ſubſtance in another.

I have ſaid, that ſeeds contain in them not only the firſt rudiments or origin of the future plant, but alſo a mealy ſubſtance, and ſome of them an oil; which mealy ſubſtance is deſigned by Nature to nouriſh and ſupport the young plant, until its roots are ſo far grown out of the ſhell that they can draw nouriſhment from the earth. But, upon examining the ſeeds of the Cotton tree which

grows in Perfia and Bengal,* and which feeds are found among the cotton brought to us from thence; I was aftonifhed to fee the variety of Nature's operation in the formation of this feed. For, upon opening thefe feeds, of which feven, eight, or nine are contained in one fhell or hufk with the cotton, I did not find any thing of a mealy nature or fubftance, but the oblong round figure of the feed is only caufed by four diftinct leaves, with wonderful exactnefs doubly folded together, and as it were embracing and defending that part from which the ftem or root will proceed. I caufed a drawing of this to be made, fomewhat magnified, in order to fhew the leaves, with their fibres or veffels, and alfo certain dark round globules which are to be feen on the leaves: thefe are fhewn at *fig.* 24, D E F G. When I cut thefe globules open as exactly as I was able, the pieces cut off exhibited the brighteft grafs-green colour that can be imagined; a particle cut thicker was of a darker green; and the globule viewed entire was of a deep green, verging to a black colour. a b c, Are three pins, with which the leaves are fpread open. The fubftance of thefe leaves was compofed of exceeding fmall globules, of a kind of grafs-green colour. I enquired of feveral perfons who had been in India, what was the form and nature of the leaves of the Cotton tree, but among all to whom I addreffed myfelf, I only found one gentleman and lady, who told me that the leaves of thofe trees were marked with fpots. Here we fee the wonderful regularity of Nature's works, namely, that in the feed of this tree there is not only formed a perfect young plant, but the leaves in that young plant are dotted with points or fpots, in the fame manner as the leaves growing on the tree. In the figure, D E is that part from which the ftem and root will be produced, and, upon cutting it open, I found in it a very few grafs-green globules.

* The Cotton tree is now cultivated in the Weft Indies, where it forms a confiderable branch of trade.

Thefe obfervations recalled to my mind (for I compare thefe feeds with thofe eggs which I have heretofore taken out of the bodies of fome infects) that in fome of the eggs fo extracted, I did not find any fubftance deftined to the nourifhment of an animal, but, inftead of it, perfect and living animals. For in like manner as in thofe eggs, while yet in the parent's body, a perfect young one is formed, fo the Cotton tree produces in its feeds not only the leaves of the young plant, even while the feeds are yet on the tree, but that part in the feed from whence the root and ftem are to be produced, is in this feed uncommonly large.

And in like manner as with regard to the eggs of the infects, I have mentioned, that as foon as the young one has broken the egg, it immediately comes forth and runs, creeps, or fwims about; fo the leaves in this feed having received the moifture requifite for their growth, do fwell and enlarge themfelves until the thin fkin wherein they are inclofed burfts or cracks, and the leaves then expanding, that part whence the root and ftem will proceed grow to a fufficient length to draw its nourifhment from the earth. In a word, fome infects are perfect in their eggs before they are voided by the parent, and the plant in the feed of the Cotton tree is perfect, and requires no nourifhment to be provided for it when it is of fufficient maturity to quit the tree.

I cut a flice tranfverfely from that part whence the root and ftem derive their origin, and caufed a drawing of it to be made from the microfcope, for no other reafon than becaufe the internal part containing the veffels, which in others is of a round or oval figure, is in this a figure of eight fides, four of which are fomewhat curved, as at *fig.* 25, A B C D, which was full of pores: the part furrounding it, which appears at E F G H, was very white, and in it I could not difcern any pores. The outward part or compartment, I K L M, both in pores and in its white colour was fimilar to the internal one, with this difference only, that the pores in the inner one were fomewhat larger. And though the greateft

part of the feeds which I examined were very old, yet I found that this young plant contained in it a confiderable quantity of oil. Some other feeds I opened, in which the young plant was fo frefh that I thought I could make it vegetate, but I have yet met with no Cotton feeds which I could by any means caufe to grow.

I afterwards faw, in a curious gentleman's cabinet in this town, fpecimens of two other Indian feeds, in which the leaves and root of the future young plant were extraordinary large, but no oil or mealy fubftance, fo that we find there are fome feeds which contain only the young future plant or tree.

If any one is defirous of feeing the young plant in great perfection, as it lies in the mealy fubftance of the feed, let him examine the feed of the Lime tree * when ripe; for in this feed are to be feen two leaves not flattened nor folded up, but of a perfect handfome fhape, the fame as a young leaf on the tree; and through the microfcope the veffels or fibres in the leaves are as plainly to be feen, as with the naked eye we can fee them in a full grown leaf: the origin or firft rudiment of the young plant, which in this feed is extraordinary large, is placed in the contrary direction from the parent tree, whereas in other feeds it points towards the tree: this young plant, before it comes to its full growth in the feed, is not of a green colour, but when full grown it becomes of a bright yellowifh green.

* A figure of this is given in Baker's Employment for the Microfcope.

On the generation of EELS.

IT is the common opinion in this country, that Eels are produced without the ordinary procefs of generation ; a notion which I could never conceive, as I have often declared ; and I will fay, that if this were true, there is no reafon why Eels fhould not be produced in fuch quantities, as in a manner to fill all our canals.

Nevertheless, this opinion is not only entertained among the vulgar, but I have found fome refpectable and learned men inclined to favour it ; and they have gone fo far with me, as to affert, that they knew in what manner Eels were generated, which they imagine to be as follows :

If in the month of May, two turfs of grafs be taken and laid on each other with the graffy fides together, and before fun-fet be placed in the water, fo that the grafs of thofe turfs be even with the furface of the water, and if the dew fall copioufly that evening, upon taking up the turfs, the following morning, feveral minute Eels will be found in the grafs, which they fuppofe proceed from the dew; in confirmation of which they add, that if no dew has fallen, there will be no Eels found.

But upon examining this matter, we muft confider, that in very windy weather no dew falls, and fmall Eels in cloudy weather, get to the bottom of the water ; but the warmer and calmer the weather, the more is the dew that falls : moreover, thefe minute Eels in warm weather, fwim or creep among the leaves and greens on the furface of the water, and do moft probably, at the fame time, creep among the blades of grafs near the furface , and this, I fuppofe, has given rife to the notion of their being generated in this manner.

In farther reflecting on this fubject, it occurred to me, that I had often in fummer time, feen boys by the fides of the ditches round about this town, with fmall twigs, which they dipped among

the leaves ſwimming on the ſurface of the water, and, twiſting the twigs round, ſo as to lay hold of thoſe leaves and draw them out of the water, they pulled out with them many minute Eels; and among thoſe, I have ſeen many young Eels of different ſizes, ſome of them remarkably minute and ſmaller than we ever obſerve the young fry of fiſhes which are produced from eggs; and this confirmed me in my opinion, that Eels are not always bred in the ſame month or ſeaſon of the year.

In order farther to inveſtigate this ſubject, I, at ſeveral times, diſſected ſome large Eels, examining particularly that part of the belly where one expects to find ſome appearance of young, (and I am well aſſured, that Eels of every kind, without diſtinction, do breed young ones), in the inteſtines of which I found many different kinds of animalcules; and afterwards I examined ſome Eels without diſſecting them, by compreſſing the belly near the aperture into the inteſtines: from one of theſe there iſſued a ſmall quantity of thick liquor, which being diluted and placed before the microſcope, I plainly ſaw that it contained animalcules, which in ſhape, length, and thickneſs, exactly repreſented Eels, but ſo minute, that in my opinion, they were not a fiftieth part the thickneſs of a hair.

After this, upon reflecting that Eels might probably breed in the winter time, I directed a fiſherman to bring me ſome every week, beginning in the month of February.

The firſt of theſe I diſſected, and upon examining the parts where I ſuppoſed the young would be depoſited, I found nothing but ſome very minute globules, ſomewhat leſs than the globules of blood, and of theſe ſome were of an oblong ſhape: proceeding in my obſervations, I found theſe kind of globules more and more extended in length, till I ſaw them of the perfect ſhape of Eels, and at laſt I extracted a great number of minute Eels, lying together in a tranſparent liquid, which, through the microſcope, appeared as perfectly formed as minute Eels viewed with the naked eye.

This ſpectacle gave me great pleaſure, partly becauſe, after taking ſo much pains, I had now diſcovered the manner of the propagation of Eels; and partly, becauſe this was a complete anſwer to thoſe who ſaid behind my back, " Since Mr. Leeuwenhoek is en-" deavouring to eſtabliſh the regular generation of all animals, let " him ſhew us in what manner Eels are bred."

The ſight of theſe minute and perfect, though unborn Eels, which were, as I have before ſaid, not a fiftieth part the ſize of an hair, I exhibited to ſeveral of my acquaintance, who wondered to ſee ſuch minute creatures ſo completely formed.

I purſued my obſervations on this ſubject till the months of Auguſt and October, till I could barely ſee theſe minute Eels with the naked eye, and I judged that they might at that time be brought forth into life by the parent.

It is by ſome aſſerted, that from the ſkins of Eels thrown into the water ſmall Eels will be produced. This may perhaps be true, though not, as they imagine, from the corruption or putrefaction of thoſe ſkins, but in the following manner. Let us ſuppoſe that among a ſcore or more Eels which have been ſkinned, one of them may have ſome minute young ones in its belly, which, by the preſſure in the operation of ſkinning, might be ſqueezed out and remain adhering to the ſkin; thoſe minute Eels, when thrown into the water with the ſkin, may there find food and grow to their full ſize; and by this means, if Eel-ſkins are thrown into a newly made canal, Eels may thereby be bred therein.

On the multitude of eyes or optical organs in the eye of a Beetle. The optic nerves of each of the optical organs in the eye of a large Fly, particularly defcribed: alfo the brain of a Gnat; and the nature and probable ufe of the hairs on the feet of Flies and Crab-fifh.

I HAVE formerly made mention of the great number of optical organs, or eyes, with which fmall flying animals, such as the dragon-fly,* or libella, the common fly, and others, are furnifhed; and I have often fhewn thofe eyes to fome curious gentlemen, when they came to vifit me, to their great delight, particularly when they found that objects might, with the greateft clearnefs, be difcerned through each of thofe optical organs, to the number of feveral hundreds at a time.

Among other inftances, I, upon a certain occafion, exhibited to feveral Englifh gentlemen of rank, the great number of optical organs which are found on the tunica cornea, or horny part of a Beetle's eye; at which fight thofe gentlemen were aftonifhed, and the rather, as it is a kind of proverbial expreffion in England, when they would reproach any perfon with blindnefs or ftupidity, to fay, "He is as blind as a Beetle," under a notion that that animal is void of fight.

With regard to this creature, I began by feparating from the head that part which is commonly called its eye, and, having cleared away the blood veffels, and other matter adhering to it, I placed it before the microfcope, and then I faw that the protuberance, or rifing of this eye, was not a perfect hemifphere, being rather more extended in length than in breadth.

* This creature is vulgarly called, particularly by children, an horfe-ftinger; but very erroneoufly, for it has not a fting, nor any weapon of offence, that is discoverable by us.

I alſo made a computation, as accurately as I was able, of the number of optical organs which were to be ſeen, in a row, in the largeſt ſegment, or longeſt ſide of the eye, and I found the number amount to ſixty; and if we ſuppoſe that, in the ſmalleſt ſegment, or narroweſt part of a Beetle's eye, there are forty ſuch optical organs in a row, theſe ſixty and forty, added together, make one hundred; the half of which, being fifty, is what we muſt take to be the number of eyes in the hemiſphere which compoſes a Beetle's eye, if we conſider ſuch eye to be of a ſpherical ſhape.

Now, if we ſuppoſe that the two eyes of a Beetle do, together, make up a perfect ſphere, and that in a great circle* on ſuch ſphere, one hundred optical organs are placed, we ſhall find, according to this rule laid down by Metius, "As 22 is to 7, ſo is the "quadrature of the great circle to the contents of the ſuperficies," and the rule being applied as follows:

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22 —— 7         10000            100   the great circle.
                    7            100
                -----            -----
            22) 70000 (3181      10000 Quadrature.
                -----
                 ·40
                 180
                   ·40
                   ---
                    18
                   ---
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the reſult is, that if each eye in a Beetle's head is taken as an hemiſphere, the two will make up a ſphere containing, on its ſurface, 3181 optical organs or eyes.

I have cauſed a drawing to be made of the eye, or rather of the

* It has been mentioned in another place, but is here repeated to ſave the trouble of turning back, that by the great circle is meant, the largeſt which can be drawn upon a globe, or ſphere, or, in other words, a circle, whoſe diameter is a line paſſing through the centre of ſuch ſphere to the ſurface.

multitude of optical organs compoſing the eye of the Beetle, as far as the limner was able to diſcover their ſtructure by the help of the microſcope, with intent, partly to exhibit the great number of thoſe optical organs, and partly to ſhew that each of theſe organs is of a convex ſhape; not that I would be underſtood to mean, that every one of theſe optical organs is a portion of a ſphere;* for, if ſo, the Beetle would not be able to diſcern objects at any diſtance, but that each of them is more flattened than ſpherical.

Upon exhibiting this object to the limner, he compared theſe convexities, or protuberances, to thoſe round buttons which are at preſent uſed in men's clothes, and made of that metal commonly called Prince's metal.

Plate XIV. *fig.* 1, A B C D E F, repreſents a part of the tunica cornea, or horny part of the Beetle's eye. A B C denote that part which was contiguous to the head. D E F A repreſent the greateſt part of the longeſt row of eyes on the ſuperficies of the tunica cornea; in which, from D E F to A, I counted more than ſixty optical organs; and, between the ſame letters, may be ſeen how each of theſe optical organs riſes with a kind of protuberance.

It is well known, that when any object is placed before the microſcope, it muſt be ſo adjuſted that it may neither be too far removed from, nor brought too near to the focus of the glaſs, for, in either of thoſe caſes, the object cannot be ſeen diſtinctly. For the ſame reaſon, if any perſon is deſirous to contemplate objects through thoſe optical organs which are in the tunica cornea of the Beetle, he will find it neceſſary to allow a ſomewhat greater diſtance between the tunica cornea and the focus of the magnifier, ſo that the focus of each may (if I am allowed the expreſſion), become united in one point, as we do when two convex lenſes are placed one before the other in a frame: and, by this means, he will ſee the object multiplied ſeveral hundred times, by reaſon of the great

* I addreſs myſelf here to thoſe who have ſome knowledge in optics.

number of optical organs in the tunica cornea, but all wonderfully minute; for the fteeple of the new church in our town, the fize of which, and its diftance from my houfe, I have mentioned elfewhere,* when viewed through thefe optical organs, appeared no larger than the point of a very fmall needle.

Hence, it appears, how greatly thofe perfons are miftaken who fay, that a Beetle is blind; and how much perfection is to be found in the organs of fight of fo fmall and fo defpifed an animal, to fay nothing of the other parts of its body; but which creature, whoever fees, immediately crufhes with his foot, as loathing the fight of that black creeping thing.

In the month of Auguft I faw, fitting on a glafs, at the backfide of my houfe, a Fly almoft as large as a bee, which fpecies of Fly, though not very numerous, I obferve every year in the fame place.

I diffected the tunica cornea of both eyes of this Fly, and, on examination, I found them to be covered with a great number of wonderfully minute hairs, which did not cover the organs of fight, but were placed in the intermediate fpaces between them.

Moreover, I took out of the infide of the tunica cornea, that matter or fubftance with which it was filled, in order to examine it by the microfcope, becaufe, till that time, I never could clearly fatisfy myfelf to what end this fubftance was created, and the rather fo, as, upon viewing it, I judged that it confifted of a collection of threads or fibres. Upon fpreading this a little afunder, to examine it more accurately than I had before done, I faw that all thofe particles which I had before confidered as a collection of threads or fibres, were nearly of the fame length, but one of the ends of each fomewhat thicker than the other, and the thicker end rounding at the extremity.

Upon repeatedly, and more carefully, examining this fpectacle, I was, to a certainty, affured that every one of that great quantity of

* See vol. I. Effay on the Silk-worm, page 62.

particles like threads which prefented themfelves to my fight, were no other than optic nerves; and that the larger and rounding extremity of each of thefe nerves was deftined to occupy the fmall cavity of each optical organ on the infide of the tunica cornea; fo that in a word, I may venture to pronounce, that every optical organ feen in the tunica cornea is provided with its particular optic nerve.

As to the other extremities of the optic nerves, which, being fituated towards the inner part of the head, are fmaller in fize: this muft, I think, neceffarily be the cafe, partly becaufe the tunica cornea dilates itfelf in a fpherical form, whence it neceffarily follows, that the optic nerves as they tend towards the internal part, muft by degrees grow fmaller, and partly becaufe the place where they terminate muft be fmaller than the fpace within the tunica cornea. And who knows, whether that part in which the optic nerves fo terminate, may not be the brain itfelf, not yet difcovered?

In order the better to fatisfy the curiofity of others on this fubject, I placed fome of the optic nerves of this fly upon a glafs, before the microfcope, which I delivered to the limner, in order that he might make as accurate a drawing of them as he was able.

Fig. 2, G H fhews two of thefe optic nerves, the part H was placed near to, or rather within the cavity of the optical organ; the other end G, towards the internal part of the fly's head.

In *fig.* 3, at I K L are feven of thofe optic nerves, the larger ends of which were alfo placed next the tunica cornea.

Fig. 4, M N O P Q, fhews a great number of optic nerves heaped together, the upper ends of which, as feen at the letters N O P, were placed next the tunica cornea. When thefe nerves are feen in fuch heaps as here pictured, their true length cannot be diftinguifhed, but when lying together in a fmall number, and a moderate fized parcel, the light may eafily be feen through them, they being fomewhat tranfparent. All this is fhewn in the figure.

I have in the courfe of my obfervations difcovered, that all the

minuteſt particles in the fibres of fiſhes, when at reſt in their natural ſtate, are, as it were, contracted in various annular folds or wrinkles ; but when put in motion or extended for any purpoſe, then all thoſe wrinkles and annular foldings are opened or ſmoothed, and the fibre becomes longer.

The ſame kind of annular wrinkles I alſo diſcovered in the optic nerves of the fly now under conſideration ; whence we may conclude, that every one of theſe optic nerves is endued with the power of alternate extenſion and contraction equally with the optic nerves in a human eye. For it muſt needs be, that when we move our eyes from one ſide to the other, their optic nerves will be more extended than when we look ſtraight forwards.

Having made this obſervation, I aſked the limner whether he could follow with his eye thoſe circular marks or lines in the optic nerves, and, upon his anſwering, that he could very plainly ſee them, I deſired him to imitate them as nearly as he was able in his drawings, and this is to be ſeen in *fig.* 2, and 3.

Now if this fly had been dead ſometime before I had diſſected the eye, and taken out the optic nerves, I am very ſure, that in that caſe, I ſhould not have diſcerned any of thoſe wrinkles or contractions. In like manner we daily obſerve in fiſhes, that after they have been dead ſometime, their fleſhy parts, if cut aſunder, do not become ſhorter, which our people call *krimpen**, and conſequently thoſe parts of the fiſh are neither ſo firm, nor ſo grateful to the palate, as when they are cut aſunder while the fiſh is alive.

Upon diſcovery of ſo many wonders, and of ſuch perfection in the eye of a fly, we are again compelled to cry out ; How little do we know ? And if this is ſeen in the eye of ſo large a fly, it muſt

* *Krimpen* in Dutch ſignifies to contract or gather up in folds, whence the phraſe to crimp fiſh is plainly derived; which cruel practice ſeems alſo to have been imported from Holland. And it must be confeſſed, that with all Mr. Leeuwenhoek's abilities as a philoſopher, he ſeems not to have had much ſenſibility of compaſſion for the creatures which he ſubjected to his examination.

be underſtood, that the ſame obtains in the ſmalleſt, and which I have ſeen in the diſſection of common flies.

At the time the limner was making theſe drawings, I took a ſmall Gnat of that ſort which is not troubleſome to us, having no ſting: the head of this Gnat I cut off, in order to extract its optic nerves, but I could not, after ſeveral trials, ſee them diſtinctly. While I was busied in this attempt, I frequently ſaw the brain in the Gnat's head ſurrounded with a great many veſſels, which I judged to be blood veſſels: and though it ſhould ſeem ſcarcely poſſible to extract the brain from this creature's head, without deſtroying the texture of the brain and its veſſels, yet, I at length ſucceeded to my wiſh, and, without their being much injured, placed the whole before the microſcope; this I delivered to the limner, that he might make a drawing of it, and the rather, as I had been told by a gentleman of eminence, that a certain perſon, whenever my diſcoveries were the ſubject of converſation, was accuſtomed to ſay, that the experiments stated by me could not poſſibly be performed; becauſe (as that ſame perſon pretended), the inſtruments uſed by me, however exquiſitely conſtructed, could not be at all capable of performing the diſſections I deſcribed. But I little regard theſe malevolent inſinuations of mankind, and perhaps this very perſon is one of thoſe who would be glad, if he was able to perform the like operations.

Fig. 5, R S T, repreſents the brain taken out of the head of this Gnat, and alſo the veſſels, which in part ſurround it, and in part pervade the ſubſtance of it, as nearly as the limner could follow them in his drawing; for while he was employed about it, he frequently declared that he could by no means repreſent in the drawing all the veſſels which he ſaw.

The flies before mentioned, have the extremities of their feet covered with an incredible number of hairy parts, by the help of which they are better able than other flies to climb up a glaſs though it be ever ſo free from impurities or irregularities, of which they might take hold. I have therefore often placed the feet of thoſe flies before

the microſcope, in order to view the means by which they can faſten themſelves to the glaſs and run up it; and I have, for ſome years paſt, thought that I could diſcover that theſe hairs, were each of them provided with crooked parts like hooks, by the help of which they can take the firmer hold on glaſs, but which parts have never, to my knowledge, been deſcribed by any perſon, though the figures of thoſe hairs may be ſeen in many authors.

In further proſecution of this enquiry, and revolving the ſubject in my mind, a thought occurred to me reſpecting thoſe large crabs, which are ſometimes brought to this town for ſale, and are, as I am informed, caught among the rocks in Norway; but not that I would at all compare theſe crabs to flies, or that kind of animalcule, otherwiſe than by analogy, for the feet of thoſe crabs are covered with many hairs. I therefore determined to inſpect them, and examine the hairs, particularly thoſe on the hind feet; for thoſe feet are not furniſhed with claws, or other weapons, to graſp hold of any thing like the fore feet, but only with one ſtraight claw, and with many ſhort hairs.

Upon placing these hairs before the microſcope, I ſaw, not without admiration, that many of them were provided with a double row of parts like teeth, placed in very exact order beſide each other, in like manner as if we were to imagine the back of a knife cut into a double row of teeth or notches. I have cauſed a drawing to be made of one of theſe hairs, in order to ſhew its wonderful formation, which I am perſuaded is intended for this purpoſe, that when the crab is climbing up the rocks, he may be enabled by this aſſiſtance to fix his feet firmly on the rocks or ſtones.

Fig. 6, M N O P Q R, repreſents this hair, in which thoſe tooth-like parts are very plainly to be ſeen, but only one ſide of the hair is here viſible, and theſe teeth in the middle of the hair are longer, as from Q to P, and from P to R they grow gradually ſhorter.

Fig. 7, S T, repreſents a part only of the hair, but in ſuch a poſition that the two rows of teeth in it are conſpicuous.

Upon this ſubject of crabs, although I have ſufficiently exhibited to the learned world, the circulation of the blood in various animals, and have demonſtrated that it is uniformly and regularly performed after the ſame manner in all, yet, as I have met with perſons who did not heſitate to deny this to be the fact, I will here relate my obſervations on two ſmall live crabs, about an inch in diameter; and, judging that the extremities of their two ſmalleſt, or hind feet, which were very broad and thin, would be proper objects to diſcover the circulation, I placed one of them before the microſcope, and immediately ſaw an incredible number of particles of blood, which appeared to be globules; theſe, though not red, were of a darker colour than the liquor they floated in, and they were running along a blood veſſel which might be called a vein, with ſuch ſwiftneſs, and ſo great was the number of particles, that it could not eaſily be conceived but by an eye-witneſs. In fact, I cannot compare the appearance of theſe round particles of blood to any thing better than by ſuppoſing we were to look through a large opening, or a window, at a fall of ſnow, violently agitated by the wind; nor do I remember that I ever ſaw the blood driven through the veſſels with ſuch ſwiftneſs. It was alſo a moſt delightful object to behold this large blood veſſel, on each ſide of it, croſſed by ſmaller veſſels, in which the blood was driven forward with equal ſwiftneſs.

Upon changing the point, or place of view, I there ſaw the blood in equal agitation, and in no leſs a number of veſſels.

Moreover, I happened to ſee a veſſel in which the blood could not proceed in its regular courſe, but yet was in continual motion to and fro, and all within a ſpace not larger than the thickneſs of an hair; whence I concluded, that, in the ſmalleſt branches, where this veſſel aroſe, and in which the circulation was completed, the blood was ſtagnated, and thence its regular courſe in this veſſel impeded.

The pleaſure I derived from this ſpectacle was ſo much in my mind, that I was induced to a farther examination of the ſubject, and

to beſtow ſtill more attention on it, by examining the hind feet of many ſmall live crabs. In one of theſe I ſaw, by the microſcope, not only a large artery, about the ſize of an hair, which, before it came to the extremity of the foot, divided itſelf into ſeveral ſmall branches; but I alſo ſaw a great number, both of arteries and veins, croſſing each other; and, on turning my eye a little towards the ſides of the foot, I there ſaw the blood poured into ſo many minute veſſels, that the ſubſtance which we ſhould call the fleſhy part of the animal, lying between the ſkin or ſhell of the foot, ſeemed to conſiſt of nothing but blood veſſels; ſo that when I ſhewed this circulation of the blood to a certain learned phyſician, he frequently exclaimed, "O how wonderful is this!"

The pleaſure which this object afforded me was the greater, becauſe the globules of blood, driven through all the veſſels, being fewer in number, were much wider aſunder than the globules in the blood of terreſtrial or aquatic animals which are of a red colour; ſo that, I am well aſſured, the globules in red blood are twenty-five times more in number than thoſe, in the ſame ſpace, in the blood of a crab.

After I had ſeveral times repeated my obſervations on theſe minute crabs, and my ſight was in a manner wearied with the ſpectacle, however pleaſing, I had a mind to ſee the blood when drawn out of the veſſels, and the rather becauſe the ſerum or fluid in which the globules were ſwimming, was ſo very tranſparent, that it would be impoſſible to diſcern the circulation without the aſſiſtance of thoſe globules.

The artery in this crab's foot was too ſmall to be opened in the ordinary way; I therefore determined to cut off a ſmall part at the extremity of the hind foot with a pair of ſciſſars, and then to collect the blood as it firſt iſſued forth, and place it before the microſcope. This being done, I ſaw in the ſmall portion of blood which iſſued from the artery, the globules of blood for ſome time continue their circular motion.

Moreover, I examined the artery which I had thus cut aſunder, in that part of it which was near to the animal's body, and where it was alſo divided into many ſmaller branches, in order to ſee whether the circulation was there continued, and with great pleaſure I ſaw, that the courſe of the blood was not interrupted, only it proceeded with a ſomewhat ſlower motion.

Hence I plainly perceived, that when any part of a limb is cut off, the circulation of the blood will continue in the veſſels that are not wounded, eſpecially if the effuſion of blood from the veſſels which are cut aſunder can be ſtopped. And we may alſo, from this experiment, be well aſſured, that there is no artery, which is not throughout the whole of its courſe divided into ſmaller branches, whereby it by degrees becomes ſmaller, until the very ſmalleſt ramifications return back towards the heart, and conſtitute what we call veins.

A portion of the blood taken from this crab's foot I examined by the microſcope, to diſcover, as far as I was able, of how many parts each globule of blood was compoſed; and I concluded that each globule was compoſed of ſix diſtinct globules, as in the human blood: moreover, I ſaw that the globules of crab's blood, when brought into contact, coagulated in the ſame manner as the blood in an human body, though, till this time, I had been of opinion that the blood of fiſhes would not be diſpoſed to coagulate, becauſe we do not obſerve any heat in it.

I expoſed a portion of the thinneſt part of this blood to evaporate in the open air, in order to ſee whether, in that thin part called the ſerum, I ſhould diſcover any particles; and, in doing this, I ſaw ſo many particles, which, to my eye, appeared ſpherical, that the whole ſubſtance ſeemed compoſed of them and no other, and, upon ſtirring the ſubſtance with the point of a needle, thoſe particles came still more diſtinctly into view.

Moreover, I diſcovered a great number of ſaline particles, which, upon the leaſt application of heat, coagulated in irregular parts.

But in feveral places I faw a great number of faline particles lying near together, of a more regular fhape, and having fix fides: but, with all the attention I could beftow, I did not difcover a fingle particle in crab's blood refembling the figure of our common falt; fo that it is plain, to me, that the fea falt does not enter into the fubftance of the bodies of fifhes.

I have caufed a drawing to be made of this crab, in order to fhew the fpecies wherein I faw the circulation performed in fo many veffels, and which is well worthy the attention of the curious.

Fig. 7, A B C D E F, reprefents this crab, and at A B and E F are fhewn the two external joints of each hind foot, in which I have faid the circulation can be feen in fo great a number of fmall veffels.

I then examined the parts which to the naked eye feem hairy, and with which hairs the joints A B and E F are covered, in order if poffible to difcover the circulation there; but the veffels compofing thefe hairy parts were fo minute and flender, that not a fingle globule of blood could pafs through them, and confequently it was impoffible for me to fee the circulation in thofe parts.

Of certain animalcules bred on the leaves of the Willow, and which produce the knobs or excrefcences frequently feen on thofe leaves.

THERE is a fort of Willow in this country, the leaves of which are of a deeper green, and of a larger fize than others of that tree; thefe Willows are for the moft part planted for the fake of their young fhoots, which are ufed in hufbandry, and gardening for binding hedges, fences, and branches of trees together, by reafon that they are very flexible, and at the fame time very tough; they are therefore called by us *Zeem-teenen,* which may be expreffed in Englifh, by the words leathery twigs.

Upon the leaves of thefe Willows, I have often obferved certain knobs or rifings, and having gathered fome of thofe leaves, and opened the knobs, I at feveral times found that they contained more than one kind of worm or maggot; but none of thofe I at firft faw, appearing to be full grown, I opened fome others of the knobs very gently, and, where I faw a maggot inclofed, I ftopped up the opening I had made, and put the leaf into a glafs tube, that the animal inclofed might there perfect its growth, and undergo its change of ftate: this however I did not fee any of them arrive at, though at the fame time, I had obferved many of the knobs vacant, and with a fmall hole in them, through which the maggot had iffued, leaving the cavity within, partly filled with the excrements it had voided.

Plate XIV. *fig.* 8, A B C D E, reprefents one of thefe Willow leaves, on which are feven of the knobs or excrefcences I have mentioned: in fome of thefe a hole is to be feen, as fhewn at the letters

F G H ; at the letter K appears a knob, wherein, upon opening it I found a maggot.

I alſo obſerved many dead maggots lying among the knobs, and upon ſearching after the cauſe of this appearance, I found at length a ſmall maggot ſticking cloſe to one of a larger ſize, which ſmaller maggot ſeemed to have no power of moving itſelf from place to place, each extremity of its body, particularly the hind part, terminating in a kind of point. This maggot, with its mouth, had pierced into, and was fixed on the body of the larger one ; and from this and many other instances which I ſaw of the like kind, it appeared to me, that this ſmall maggot was ſupported by preying upon the larger one ; and hence I conſidered that were it not for food drawn from thoſe or the like kind of maggots, theſe ſmaller ones would be deſtitute of ſubſiſtence ; for they are not provided with any limbs or organs by which they can faſten themſelves to the leaves of trees.

But how theſe laſt mentioned maggots could find their way into the internal parts of the knobs or excreſcences was ſtrange, and to me entirely undiſcoverable, otherwiſe than by conjecture. I ſuppoſe, firſt, that the larger maggot was produced from an egg laid by ſome fly on the leaf of the Willow, and upon its biting or gnawing the veſſels in the leaf, a liquid ſubſtance iſſued forth, ſurrounding the maggot on all ſides, and in hardening formed the knob or excreſcence we obſerve on the leaf: ſometime afterwards another, and a ſmaller fly might place itſelf on the knob, and by means of ſome weapon or piercer, bore a hole in the knob and lay therein an egg, from which egg, the ſmaller maggot might be produced.

About the middle of July I repeated my obſervations on theſe maggots, and found many of them almoſt arrived to their full growth : and ſome actually changed into aurelias or cryſales* : I alſo found

* The tranſlator has taken the liberty to expreſs the plural number of the word cryſalis by this termination, inſtead of adding another s to it, with an apoſtrophe between the letters, as is ſometimes done.

ſome of thoſe minute voracious maggots, (voracious I may juſtly call them, ſince they prey upon a maggot not leſs than fifty times their ſize), which I judged were ſo far grown, that without taking any more food, they would ſoon be changed into flying animals.

I had, at firſt, no doubt that theſe aurelias or cryſales were of the ſame nature as thoſe of common flies, namely, that when the maggot is about to be changed into a cryſalis, its ſkin contracts, and by ſuch contraction becomes more firm and ſolid, and ſerves the animal for a ſhell or caſe during the progreſs to its change, and then we call it an aurelia or cryſalis, though it be not wrapped in ſuch a web or ſpun caſe, as we obſerve many other flying inſects to be; but upon opening theſe cryſales or aurelias, I found the maggot within them in its original ſhape, although it had lain in the aurelia ſtate a fortnight. And, upon viewing theſe things by the microſcope, I found I had been miſtaken in my former opinion, for the ſhell or caſe which I have called a cryſalis, was neither compoſed of a web nor of the ſkin of the maggot itſelf, but I was obliged to conclude, that it was formed by a concreted ſubſtance iſſuing from the leaf of the tree, for it had the appearance of fibrous or branched parts, from which there iſſued ſmaller ramifications. And I did not find a cryſalis in any of the knobs I opened, excepting in thoſe which I had kept in glaſſes: one of theſe cryſales, as it lay in the knob, is ſhewn at L.

After ſome weeks I obſerved, that from theſe cryſales certain blackiſh flies were produced, which at their tails had ſome oblong parts formed in the nature of ſtings, and another ſort of flies, rather of a ſmaller ſize, but without any ſuch appearance of ſtings.

Fig. 9, repreſents one of theſe flies the ſame ſize it appeared to the naked eye, and at the letters C D is ſhewn that part which had the appearance of a ſting, and was two thirds as long as the animal's body. But, upon placing this before the microſcope, it did not at all appear like a ſting, for it was covered with a great number of ſmall hairs, as ſhewn in *fig.* 10 at the letters A B, and no aperture was to be diſcovered in it. Whereupon I began to conſider that

perhaps this was only the cafe of the real fting, and, upon opening it, I found a fting, notched at the point like a faw, of which I have given a drawing in *fig.* 11, E F.

When I had attentively viewed this for a confiderable time, I thought it feemed to contain another fting within it; I, therefore, endeavoured to open it, wherein I fucceeded to my wifh, and out of it, at the place marked E F, in *fig.* 11, I took two ftings, both of the fame make; a fmall piece of one of them is fhewn at *fig.* 12, G H: each of thefe was jagged like a faw.

Hence it appeared, that thofe indentings or teeth, reprefented in *fig.* 11, at E F, did not properly belong to that part, but to the ftings enclofed in it; and, when thofe ftings were taken out, it appeared neceffary to conclude, that the part which I had taken for the real fting was, in fact, only a fecond cafe for the two inner ones: in this fecond fheath, or cafe, as drawn in *fig.* 13, is fhewn at I K, the cavity wherein the two ftings lay; and farther, in *fig.* 12, another cavity is to be feen: all which things, put together, give reafon to conjecture that, in each of thefe cavities, there may be contained fome acrid juice of a poifonous nature.

From the foregoing obfervations, we may eafily conceive, that thefe minute flies do not merely lay their eggs upon the furface of the leaves, but that, with their ftings, they perforate the membranes of the leaf, and depofite an egg in the cavity; the maggot iffuing from which, in biting the veffels, caufes the juices of the leaf to iffue forth, which form around it thofe knobs, or excrefcences, I have defcribed.

I obferved, among the dead maggots in thefe knobs, two white oblong particles, but fo very minute as to efcape the view of the naked eye; thefe I concluded to be eggs, for there was, in no part of them, any refemblance of a maggot, and, the next day, two maggots iffued from them, exactly of the fame make with thofe which I have faid preyed on the larger maggots.

After this I took one of thofe voracious maggots from the body

of a dead one, on which it had been feeding, and placed it on a living maggot, into which it immediately plunged its mouth, and, notwithftanding all the efforts the other made, by extending, contracting, and twifting its body, to fhake it off, it ftill remained fixed to it. One of thefe voracious maggots, when fully grown, appeared no larger, to the naked eye, than is fhewn at *fig.* 14.

I caufed a drawing to be made, from the microfcope, of one of the maggots I have been defcribing, when changed into an aurelia or cryfalis. *Fig.* 15, A B C D E F, reprefents this cryfalis, which, in the evening was a maggot, and, the next morning, was changed to the figure here reprefented, and, in its change, had put off an exceeding thin pellicle; and in like manner as the body of it, when a maggot, was compofed of many parts like rings, fo, when a cryfalis, it retained the fame annular or infected form. In this creature, not only the feet, but every fingle joint in them, might plainly be feen.

At the letters G C and D G, are fhewn the two horns, and though thefe were inclofed in the fame thin membrane with the body, yet every joint in them could very plainly be feen, and they were fully formed, and distinct from the body.

This maggot before, and for fome time after, its change, was of a perfect white, but, after fome days, the eyes, which may be feen at B and E, and each of them containing many optical organs, assumed a dark colour, inclining to a blue.

I had many of thefe maggots, and I watched them very narrowly, to difcover in what manner their transformation from a maggot to a cryfalis was performed; but the change, when it took place, was made in fo fhort a time that I never could get a fight of it.

A defcription of fome fpecies of minute Infects, found in frefh water, in a Letter to Signor Anthonio Magliabechi, at Florence.*

To the illustrious, learned, and famous Signor Anthonio Magliabechi.

THE very obliging letter with which you have honoured me, (to which were added what has lately been publifhed in Italy by the learned, containing the curious invention of inclofing gunpowder in a bag, and computing the force which fuch bag would refift before it burft) is come to my hands; and I beg you to be affured that it was moft acceptable to me: and for the favour of your letter, as well as the communication therein contained, I defire to return you my grateful thanks.

In reliance on your accuftomed friendfhip, I have been induced to impart to you my obfervations and reflections refpecting fome very minute animalcules, hitherto very little noticed, which are found in the waters of this country.

Being employed in fearching after certain animalcules, which I expected to find in thofe ditches or canals which divide our fields, I faw various fpecies of creatures, but none of thofe which I was then in fearch of. Among thefe I obferved certain animalcules, within whofe bodies I faw fo quick a motion as to exceed belief; they were about the fize of a large grain of fand, and their bodies being tranfparent, that internal motion could plainly be feen: Among other things, I faw in the body of one of thefe animalcules a bright and round corpufcle, placed near the head, and in which a very wonderful fwift motion was to be feen, confifting of an alternate extenfion and contraction. This particle I concluded to be the

* Magliabechi was at this time Librarian to the Grand Duke of Tuscany: he was not lefs famous for his learning than his peculiarities, of which feveral anecdotes are related in the Adventurer.

heart, and that the rapid motion of the parts round about it, which I obferved in this animalcule, and the motion which I had feen in the others, proceeded from the action of the heart.

Upon obferving in one of thefe animalcules eight or nine greenifh particles I began to think that thofe were unborn young ones; to afcertain which I put one of the animalcules into a little water, being the quantity of five or fix drops, that I might thereby fee the young when newly iffued from the parent and fwimming in the water: but the next day, finding the animal dead, I opened its body while lying in the water, and then I not only very plainly faw moft of the unborn young ones, but I could alfo difcern the organs or limbs provided for their ufe in fwimming.

This animalcule was of fuch a pretty fhape, that I often viewed it with great admiration: indeed, I was fo much pleafed with its formation, that I thought larger animals viewed with the naked eye would, in comparifon with this, appear rudely made.* The death of this creature feemed to me to be caufed by want of food, becaufe I had feen it frequently void its excrements, whence I concluded that it required a large fupply of nutriment in proportion to its fize.

After this I difcovered another fpecies of animalcule, almoft of the fame fize with the former ones, but of entirely different make. This animalcule had a long forked tail, and each of the forked parts again divided into four parts, which laft parts were provided with various organs.

But obferving one of thefe animalcules, (though of the fame fpecies), to appear as if the hind part of its body was formed in a very different manner, I put two of them into a fmall portion of water, in order to examine them by a microfcope of greater magnifying

* This being the cafe, it feems ftrange that Mr. Leeuwenhoek has not given a figure of this animalcule; from the account here given of it, and particularly the motion of its heart, it fhould feem to be of the fpecies called pulex aquaticus, or water flea, of which a figure, as feen through the microfcope, with a defcription of the creature, will be found in Baker's Employment for the Microfcope.

power. By the help of this, I perceived that thofe parts near the tail, and which appeared like two bunches of grapes, were in fact, the animal's eggs, fo placed together, that there were about three or four eggs in breadth, and nine or ten in length, joined to each other, but of a round fhape, at each extremity, and the whole bearing the appearance of a bunch of grapes.

The eggs which adhered to the tail of thefe two animalcules, exhibited a fmall fpot in the middle of them, and round about that fpot a pellucid fubftance; whereas, on the contrary, the eggs of the other animalcule appeared to me of a greenifh colour.

The following day fome of thofe eggs, which in the middle had that fpot, were miffing, and after fome hours had elapfed, all the eggs were feparated from the tail; at the fame time I faw a number of the fhells of the eggs fwimming in the water and alfo many minute animalcules of fuch a fize as might be expected to iffue from thofe eggs. But as all thefe animalcules, though of the fame fize, did not exactly refemble the parent in fhape, I could not thoroughly fatisfy myfelf in regard to them.

Another animalcule I had placed in a glafs tube by itfelf, and the third day after it being fo put apart, its eggs began to change, each of them being marked with a fpot, and the external parts becoming tranfparent. The following day, purfuing my obfervations, I perceived that all the eggs were feparated from the tail, and a great number of animalcules of the fame fize and make as thofe I had at firft difcovered fwimming in the water, and moving themfelves flowly from place to place by ftriking on the water with certain organs they were furnifhed with.

At fight of all thefe things, I was convinced that thofe animalcules which I had before obferved, proceeded from the eggs; but, to be more certain in this refpect, I put the parent, with all the young ones newly brought forth, into a clean glafs, into which I had poured about the quantity of a cubic inch of water, in order to fee whether in that water they would increafe in fize, and undergo any change of figure: but, upon viewing them again the following day,

I ſaw but a very few of them, and after about thirty hours from the time of their being firſt excluded from the eggs, I only ſaw one of theſe young animalcules, which was adhering to the glaſs, and ſeemed a little increaſed in ſize, The next day I could not perceive one of theſe young animalcules, upon which I began to think that perhaps the parent, for want of other food, might have devoured its own offspring.

Not content with the preceding obſervations, I afterwards, at three ſeveral times, caught ſome of theſe animalcules which had eggs faſtened to their bodies, and placed each of them in diſtinct glaſſes, that ſo I might be better ſatisfied as to the nature of theſe creatures.

Among theſe I had one animalcule much larger than the reſt, and an extraordinary number of eggs adhering to it. As ſoon as this animalcule had let go the eggs, and the young ones excluded from them were ſwimming in the water, I killed the parent; that by this means I might the more certainly determine in what manner theſe creatures acquired their growth and increaſe; but my glaſs happening to be leaky, and the water eſcaping out of it, I was diſappointed.

Moreover, I had taken and placed apart another animalcule, half the ſize of the preceding one, which nevertheleſs had eggs fixed to its body, though not a fourth part ſo many in number as thoſe on the former one. After two or three days, theſe eggs were ſo far altered, that I judged the young ones would ſoon iſſue from them; but the next day the animalcule was dead, and all the ſhells of the eggs were adhering to its body, though the young ones produced from them were ſwimming about in the water.

It is well known, that all the fiſh in our ſea or rivers which lay eggs or ſpawn, and are therefore called oviparous, are, when a year old, able to propagate their kind, being then provided with roes or eggs; and that theſe eggs are, ſingly taken, as large in the ſmalleſt fiſh as in the largeſt of the ſame ſpecies; ſo that the difference in the ſize of the roes proceeds only from the greater num-

ber of eggs which they contain. The ſame is the caſe with reſpect to the minute animalcules of which I am now ſpeaking: for the eggs of the ſmaller animalcules were of the ſame ſize with thoſe of the larger ones, though the eggs of one were four times more in number than thoſe of the other.

Theſe laſt animalcules I placed in a glaſs filled with water, in which were ſwimming ſome other animalcules of the ſame kind, which were about ſeven or eight days old; at which time I judged theſe animalcules to be four times the ſize they were when firſt excluded from the egg.

On the 13th of July I had ſome few animalcules in a beer glaſs full of water, none of which had any eggs adhering to them; but the next morning I ſaw a number of eggs, of a greeniſh colour, on the body of one of them; whereupon I took this animalcule from the others, and put it into a glaſs tube, about the thickneſs of a common gooſe quill, with intent to diſcover in what ſpace of time young animalcules would be produced from theſe eggs.

In the morning of the 16th of July, I perceived ſome animalcules which had iſſued from the eggs, and others ſtill lying within the eggs: but whereas theſe eggs had before been joined together in very regular order, now, on the contrary, the entire eggs and the ſhells of thoſe from which young ones had iſſued, lay diſperſed in a very irregular manner.

In the mean time I had, on the 15th of July, obſerved another animalcule, having the lower part of its body filled with eggs, though the day before I had not ſeen a ſingle egg on it: this, therefore, I placed by itſelf in a glaſs, and, on the 18th of July I ſaw all the young animalcules come forth from the eggs, and ſwimming about in the water.

From theſe obſervations I was certain, that all theſe animalcules brought forth their eggs in the ſpace of a day or a night, and placed them on the hind parts of their bodies, in the exact and re-

gular order I have mentioned, and that, after three days more, the young within the eggs were perfectly formed.

Thefe laft-named animalcules did not live above three or four days, whence I conjectured that they died for want of food.

I determined to give a drawing of one of thefe animalcules, with the eggs adhering to its body, as nearly as the limner was able to reprefent it; for when I took one of them out of the water, none or a very few parts of it could be plainly diftinguifhed, fo that it was neceffary to make the drawing while it lay in the water.

Plate XIV. *fig*. 16, A B C D E F G, exhibits this animalcule, as it lay with its back next the limner. The letters C H I and E K L reprefent four horns with which the head was furnifhed.

At the letters B M and F N are to be feen the eggs which the animalcule, having brought forth, had placed on the outfide of its body about this part.

A O and G P, are the forked parts of the tail, which round about their extremities, fhewn at the letters O P, were each again fubdivided into four parts, which might have been taken merely for hairs; but when accurately examined, and with a good magnifier, then each of thefe parts (marked in the figure O Q and P R) appeared to be covered with a great number of wonderfully minute particles.

When the animalcule lay in this pofition, or even with its belly towards the limner, thofe limbs or organs with which it moves itfelf in the water, could not be diftinguifhed. I, therefore, placed it fo that its fide might come into view, and that thus the limner might copy them to his beft in the drawing.

Fig. 17, exhibits, at the letters *a b c d*, thofe organs or limbs, each of which confifts of eight parts, though they could not all be here fhewn; and each of thefe is compofed of various organs, part of which may be feen at *fig*. 18, at the letters *e* and *f*.

This animalcule did not ufe thefe limbs in the fame manner as

terreſtrial animals which have many feet, nor as thoſe in the water do for the moſt part, that is, by moving the feet ſucceſſively one after another ; but this creature, with great ſwiftneſs, moved all its limbs at the ſame inſtant, ſo that with a ſingle ſtroke it moved very quickly and made much way in the water.

Here we find another convincing proof, with how many perfections ſo minute an animalcule is endowed, as well in regard to the ſtructure of its body, as to its innate diſpoſition, or the inſtinct whereby it is pompted, when the eggs are fully grown within its body, to place them on the outſide of it, and to the intent (as ſeems to me) to protect the eggs from being injured or devoured by other animalcules ; and that the young ones, when in a perfect ſtate, are able to diſengage themſelves from the eggs. And who can diſcover all the farther perfections with which ſo minute and (to us) inſignificant a creature may be endowed ?

When, therefore, we ſee theſe wonderful properties in ſo ſmall and, to us, contemptible a creature ; and, moreover, figure to ourſelves in imagination what farther we can ſuppoſe concealed in it, can any one with reaſon ſuppoſe that ſo many perfections are produced ſpontaneouſly, or by chance ? But we ſhall be a thouſand times nearer the truth, if we lay it down as an axiom or indiſputable fact, that all the living creatures we behold at this day, however minute, derive their origin from thoſe which were formed at the Creation.

I have frequently declared this to be my opinion, and ſhould not now have touched upon the ſubject, were it not that I find, by experience, moſt men ſtill adhering to the old opinion, and the follies of antiquity, that many minute animalcules are produced ſpontaneously.

But if we ſee ſo much perfection, as well in the make of the body as in the aptitude to propagate its kind, in the animalcule of which I have been treating, we may, by parity of reaſoning, conclude, that the ſame perfection muſt neceſſarily exiſt in the minuteſt fiſhes

or animalcules, even in thofe whofe whole bodies are flenderer than thofe very flender particles with which each of the four parts (marked in the figure by the letters O Q and P R) are covered. For, in a word, the animalcule I have juft been defcribing may be confidered as an elephant, if compared with the moft minute of thofe fifhes or animalcules which may be difcovered in all waters.

If it be then afked, to what end fuch exceedingly minute animalcules were created, no anfwer can readily be given which feems more agreeable to the truth, than that, in like manner as we fee conftantly, that the larger kinds of fifh feed on the fmaller; as, for example, that the cod fifh preys on the haddock and other fmaller kinds of fifh; the haddock again on the whiting; thefe on ftill fmaller fifhes, and among the reft on fhrimps; and fhrimps on ftill more minute fifhes; and that this gradually prevails among all the kinds of fifh: fo that, in a word, the fmaller are created to be food for the larger. Again, if we confider the nature of our fea, abounding with fifh, yet having nothing at the bottom of it fave barren fand: ftored with various fhell-fifh, yet deftitute of every green herb; and if we, moreover, lay it down for a truth, that no fifh can be fupported on water alone, there will not remain a doubt, that the fmaller fifhes are deftined, by Nature, to be the fubfiftence of the larger.

You will excufe, illuftrious Signor, my boldnefs, in taking up your time with my remarks, of little value, fince you can employ yourfelf in much more ufeful ftudies than to read the trifles I commit to paper.

I remain, &c.

Delft, in Holland,
the 16th October, 1699.

A. van Leeuwenhoek.

ON THE MAGNET OR LOADSTONE.

HAVING at ſeveral times examined the Loadſtone, without committing my obſervations thereon to writing, I at length determined to make and note down a ſeries of regular obſervations on the ſubject.

I took two Loadſtones, each of them weighing ſome pounds, and ſuſpended them to a balance, in order to ſee what tendency they might have to iron: I then brought a piece of iron very near to the Loadſtone, but I did not perceive that the balance to which it was ſuſpended, did at all move from its poſition.

After this I took one of the before-mentioned Loadſtones, in order to ſee how much weight of iron it was capable of lifting. The firſt trial I made, was with the key of my houſe door; this the Magnet would not lift, but after I had filed off a little of the key from that part where the Magnet was to be applied, that by this means the fatty ſubſtance, left by repeated handling on the key, might be cleaned away, then the key adhered to the magnet. Upon obſerving this, I no longer wondered that the Loadſtone, when it was before applied to the iron, had not ſhewn any attractive power, as if it had not much virtue, becauſe I conſidered that this balance had been put to all ſorts of uſes in weighing.

I put a piece of this Loadſtone, about the ſize of a filbert, into a glaſs tube, and cloſed the orifice of the glaſs by the help of fire. I then brought the Loadſtone near to a ſea-compaſs, and I found that the needle of the compaſs was as much attracted by the Loadſtone, as if it had been on the outſide of the glaſs.

I then broke the glaſs, and taking that part in which the Magnet was placed, I cloſed both ends with a blow-pipe, leaving ſo much cavity or portion of air within, as, in my judgment, would be ſufficient to keep the glaſs and the Magnet in it from ſinking in

water; and by this proceeding I hoped to difcover, whether the Northern part or pole (as it is called) of the Magnet would point towards the North.

Plate XIV. *fig.* 19, A B C D, reprefents this glafs; at E F is the piece of Loadftone which I placed in the middle of the glafs, that neither of the extremities might be depreffed in the water more than the other. I then put the glafs into an earthen veffel filled with rain water, and I prefently faw the North pole of the Magnet move towards the Northern fide of the veffel; yet fometimes the glafs and the Magnet in it took a contrary direction, but which, I think, proceeded from hence, that as I often took the glafs out of the water, I might, in replacing it, put the North Pole of the Magnet higher or lower than it fhould be, and therefore that it might not uniformly move the glafs in the fame direction.

I alfo befpoke fome fmall fteel needles, fuch as are ufed for fea-compaffes, and both extremities of thefe, namely, the North and South, were rubbed with a Loadftone by a man who made it his bufinefs to prepare fea-compaffes. Thefe needles, I put into glafs tubes, clofed at the ends, and of a fize juft to float, and putting them into the water, I prefently faw the extremities of the needles tend towards the North and South; but the leaft particle of duft, or breath of air meeting the tubes did, I thought, impede their motion.

But finding, that by reafon of the length of thefe tubes, the Magnet very difficultly and flowly exerted its influence on the tubes to move them in the water, I broke the glafs again, and prepared another of fuch a fhape, that the Magnet might put it in motion more fpeedily: the ends of this glafs I also clofed with a blow-pipe.

Fig. 20, G H I K L M N O, reprefents this glafs; the letter P indicates the place where the Loadftone was put; and by heating one end of this glafs, I bent it in the direction L M, that its dif-

ferent motions in the water might be more eafily diftinguifhed. This glafs I put into a very large and broad beer-cup, in which it funk in the water as deep as the place marked K: the point M immediately veering about to the North-eaft part of the heavens, and as often as I altered its pofition, it returned to it again.

I had alfo feveral fragments of the before-mentioned Magnet, one of which weighed no more than five grains; others of them, fomewhat larger. I took three of thefe fmall pieces or particles, and prepared for them three fmall glaffes of the fhape reprefented at *fig.* 21, Q R S T, in which, at V, the particle of Loadftone is to be feen. The part Q R was added for no other purpofe than to keep the glafs upright in the water. Two of thefe fmall glaffes were fo made, that no more than the part S T appeared above the furface of the water; the third was fomewhat more elevated.

Upon putting thefe glaffes into the water, the extremity or part, T, of one of them turned itfelf towards the Weft; and as often as I changed its pofition either way, it refted not till it had returned to its firft pofition; and even when, with a quicker motion, I turned the cup round from the South towards the Weft, fo that the part, T, almoft pointed towards the North, that part immediately turned back to the Weft, and there refted. The fecond glafs pointed its part, T, towards the Eaft; and in whatever direction I moved it, it would not remain at reft, except in its firft pofition.

I was greatly furprifed to find, in thefe experiments, that fuch minute particles of Loadftone, clofely fhut up in glaffes, and moreover immerfed under water, would yet point to that part of the Heavens whereto they were inclined.*

* The uniformity of Nature's operations, in cafes analogous, will be feen exemplified, on comparing thefe appearances in the Loadftone with what has been obferved by naturalifts in

The third glaſs being put into the ſame cup with the two former, I turned it in various directions, but it did not uniformly ſettle in the ſame poſition; for its part, T, ſometimes pointed towards the South, ſometimes towards the Weſt, and then would incline a little from the Weſt towards the South. When I firſt ſaw this, I began to conclude that the fragment, though broken off from a Magnet, might yet be deſtitute of North and South poles, and, therefore, had no power to put the glaſs wherein it was placed in motion. But ſoon afterwards rejecting that idea, I thought that perhaps the North and South poles in this fragment might lie ſo as to point one of them directly upwards and the other downwards; and that if ſo, they could not by any means put the glaſs in motion. I, therefore, ſeveral times took this ſmall glaſs out of the water, and ſhook it up and down, that by ſuch ſhaking the poles or corners, if they lay up and down, might aſſume on horizontal poſition. And, after frequent trials, I found that at length this glaſs took a certain direction, and kept that poſition, in like manner as I have related of the others.

Moreover, I took an iron key, and brought it near to the glaſs, while ſwimming in the water (but ſo as not to let the key touch the water) in order to ſee whether this ſmall piece of Loadſtone, thus incloſed and under the ſurface of the water, would be put in motion by the approach of iron: but I could not obſerve the leaſt motion. Nevertheleſs, upon bringing a thick and long piece of iron very cloſe to the water and alſo to the glaſs, the glaſs inclined a little to one ſide: the ſame I alſo obſerved in the larger glaſs tube, which is pictured at *fig.* 19.

that minute aquatic animal or inſect, the Polype; which, if it be divided into many pieces, each piece will become a perfect animal, poſſeſſing all the properties of the original Polype, before divided. Mr. Baker, in his attempt towards a Natural Hiſtory of the Polype, has a curious eſſay reſpecting the diviſibility of the conſcious principle in that living creature, which ſeems applicable to the inanimate properties multiplied in this ſubdiviſion of the Loadſtone.—TRANSLATOR.

But the motion produced in the glaſſes by theſe pieces of Loadſtone was ſo feeble, that I judged if a power not equal to the thouſandth part of a grain, had been oppoſed to the ſmall glaſſes, it would have given them a different direction; for theſe particles of Loadſtone had very little virtue in them, if compared with what I have heard boaſted reſpecting other Loadſtones.

Moreover, I made another glaſs veſſel, to contain a piece of Loadſtone; the ſhape of which veſſel is ſhewn at *fig.* 22, A B C D E F G H I K, and in the cavity of this, at A B C I K, a fragment of Loadſtone was placed, as marked at L. The length of this glaſs, from C to D, was almoſt nine inches; the cavity in which part was about the thickneſs of an horſe hair. I purpoſed to make the part, D E M, with a cavity of juſt ſuch a ſize, that by means thereof a ſmall part only of the glaſs might riſe above the ſurface of the water: and, after three ſeveral trials, I made the glaſs ſo to my wiſh, that the upper part of the cavity, D E F, barely appeared above the ſurface. The reaſon why I made this glaſs of ſuch a length was, partly that I might ſee whether the particle of Magnet, when ſo deep under the water, would yet have power to turn its North pole towards the North, and ſo to cauſe the glaſs veſſel to change its poſition; and partly to obſerve whether, when ſunk ſo deep, it would be at all affected by iron brought cloſe to the ſurface of the water. This being done, I turned round the part, F G, ſeveral times; ſometimes with a quicker and ſometimes with a ſlower motion; and I always found that it would not remain at reſt until its extremity, G, pointed towards the Eaſt.

I could not make this experiment in a beer-cup, by reaſon of the length of the glaſs; therefore I took a wooden veſſel of that ſort which are uſed in this country to meaſure charges of powder for guns, and called by us, *cardoes-dooſen,* which may be rendered in Engliſh, cartridge-boxes.

After this I made another glaſs, the thinneſt part of which, ſimilar to what is pictured in *fig.* 22, at C D, was thirteen inches long:

into this I put a piece of Loadſtone, which was half as long again as thick, but before incloſing it I carefully examined, by the help of a ſea-compaſs, in what part its North pole was ſituated; and finding it to be, not at the extremity but rather towards the middle, I placed the Loadſtone upright on its end in the glaſs. The top of this glaſs, when it was put into the water with the Loadſtone in it, ſcarcely appeared above the ſurface, and, after moving it in ſeveral directions, I found that it would not remain at reſt until the North pole of the Loadſtone in it pointed to the North.

When this glaſs with the Magnet in it floated in a ſtate of reſt on the water, I took a piece of iron, almoſt eighteen inches long, one extremity of which I approached firſt to the ſurface of the water, and then to the outſide of the wooden veſſel near the bottom, about the part where the Magnet lay, in order to ſee whether by this the glaſs containing the Magnet would be put into any new motion, but I did not at that time perceive any.

After this I applied the ſame iron, not by its end but the whole length of it, as cloſely as poſſible to the wooden veſſel, and immediately I perceived that the glaſs inclined ſomewhat towards it, whereupon I ſlowly moved the iron round the box, and ſaw that the Loadſtone in the glaſs, and which was fourteen inches under water, followed the motion of the iron, ſo that in nearly a minute's time it performed a complete revolution. By this experiment the tendency of the Magnet was proved to be greater towards the iron than towards the North part of the heavens, as appeared by its following the motion of the iron. Then, after ſome interval, I placed the iron lengthwiſe, cloſe to the wooden box or veſſel, in order to ſee whether the Loadſtone would be attracted by the iron, towards the ſide next to it, which however did not happen, for it kept its poſition unmoved, excepting only that its North pole altered its poſition a little from the North.

Afterwards I made another glaſs, wherein I put a fragment of Loadſtone, which glaſs was of the ſame ſhape as that repreſented at

fig. 21, Q R S, this I put into a common beer-cup filled with water, and when the Magnet had turned to the North, and was at reſt, I took a round piece of iron, about an inch in diameter, and placed it lengthwiſe cloſe to the beer-cup, when I immediately perceived that the glaſs, with the Loadſtone in it, inclined ſomewhat to the iron; whereupon I moved the iron, as gently as I could, round about the cup, and the glaſs, with the Loadſtone in it, immediately followed the motion, but without approaching nearer to the ſide of the cup, and in about a minute's ſpace performed a complete revolution.

After this I placed the iron as cloſe as poſſible to the cup, in order to diſcover whether the attraction between it and the Magnet would be ſufficient to move the latter from the middle to the ſide of the cup next the iron, but this did not take place though they were left a whole night. From theſe things I conclude, that the attraction between the Magnet and the iron will ſhew itſelf upon the leaſt application, ſo far as to cauſe the circular rotation I have mentioned, but no farther, for very little force is required to effect this, in compariſon of that which would be neceſſary to draw the glaſs, containing the Loadſtone, through the water to the ſide of the cup. Nevertheleſs, I do believe that there might be ſome tendency in the Loadſtone towards the iron, and I have no doubt that if this Loadſtone had been larger, or of ſtronger power, it would have moved to the ſide of the cup next the iron.

Once more, I made a much ſmaller glaſs, of the ſame ſhape as laſt mentioned, in which I put a ſmall piece of Loadſtone, not much more than the weight of a grain, and I found that ſmall particle of Loadſtone to produce the ſame effect as I had before obſerved in the other pieces, and the only difference in my obſervations was, that the larger the Loadſtone the quicker were its motions.

On the Subſtance of the Brain in a Turkey, a Sheep, and a Sparrow.

IN my examination of a Turkey's Brain, I began with that part which is called the corticle;* this part, beſides the small blood-veſſels and globules found in it, is compoſed of a very pellucid cryſtalline, and (as it appears to the eye) oily ſubſtance, which, from its clearneſs and tranſparency, ſhould rather be called the vitreous or glaſſy part, than the corticle or ſhell, of the Brain. When I ſpread this into ſmall particles, I perceived to iſſue from it a ſmall quantity of a thin fluid, compoſed of very minute globules, thirty-ſix of which would not be equal in ſize to a globule of the human blood; this liquor, though it did not extend farther from the brain than about the tenth part of the diameter of an hair, was yet mixed with thoſe globules. I obſerved this fluid matter principally in the Brains of thoſe Turkeys which had been killed ſome time, whence I concluded that it had partly iſſued from the ſmall veſſels of the Brain, and perhaps that ſome of thoſe veſſels themſelves might have been diſſolved into a fluid matter. Beſides thoſe ſmall globules, there were ſome larger ones, of which I judged ſix would be equal in ſize to one globule of human blood; theſe two ſorts of globules I judged might iſſue from the minute veſſels which I might have

* That is, the ſhell or caſe, from the Latin word cortex, which ſignifies the bark of a tree.

broken, and, that, what in the animal, when alive, was a thin fluid, might become ſolid in the veſſels when cold, and exhibit the appearance of thoſe globules. I alſo ſaw, diſperſed among them, ſome other bright irregular globules of different ſizes, and ſome as large as a globule of human blood, and others larger. Among, or in the midſt of this watery matter and globules, there were diſperſed a great number of wonderfully minute blood-veſſels, and these in no greater a portion of Brain than the ſize of a large grain of ſand. Many of theſe blood-veſſels were ſo minute that (to judge by the eye) if one of the red globules in the blood of a Turkey, or other bird, were to be divided into five hundred parts, not one of theſe parts could be contained in the cavity of thoſe veſſels. For I judged, that if the ſize of the cavity of thoſe veſſels ſhould be conſidered as 1, the axis of one of thoſe globules muſt be as 8, and conſe-
8 quently, if the axis of any ſphere is 1, and the axis of ano-
8 ther is 8, then the proportion will be as 1 to 512; and though
64 theſe blood-veſſels were ſo minute, yet their colour plainly
8 ſhewed that the ſubſtance they contained was what gives the
512 blood its red colour. And, if I had not taken notice of the
faint colour which a globule of blood, ſingly taken, exhibits, it would have been impoſſible for me to know theſe to be blood-veſſels, and thus it appears that the reddiſh liquid imparts to them ſome kind of colour; this I was the more certain of, by obſerving ſome of thoſe veſſels ſomewhat larger which aſſumed a redder caſt. Theſe very minute blood-veſſels appeared of a deeper colour where three or four of them lay one on another, without any interjacent ſubſtance. From theſe appearances, I was more firmly of opinion than before, that the globules of blood, whence its redneſs proceeds, are divided into ſmaller parts, when they come to ſuch minute veſſels as they cannot enter without being divided; and I was now of opinion, that the dark colour of that part of the Brain, called the corticle, in which it differs from the white part, called the medul-

lary or marrowy ſubſtance of the Brain, proceeds from this, that the greater part of it conſiſts of pellucid particles, which are ſo cloſely joined together, that they exhibit a glaſſy or watery tranſparency, and that the dark colour is increaſed by the multitude of blood-veſſels paſſing through it. I alſo obſerved many very minute particles, which I judged to be globules, about the ſixth part of the ſize of a globule of human blood: theſe were not tranſparent, and gave the Brain a darkiſh colour, and reſpecting them I thought that, perhaps, they might have iſſued from ſome of the blood-veſſels which I had cut aſunder.

We may alſo perceive that this corticle of the Brain is, in many places, deeply mixed among the medullary parts, but when we come to ſeparate them gently, we find, between the two, ſo great a number of blood-veſſels, that they ſeem to conſtitute an entire membrane; and we ſhall find that this internal part has as many blood-veſſels as the corticle, or external part, of the Brain. Beſides theſe ſmall blood-veſſels, I obſerved in the Brain other veſſels ſo ſlender that, in my judgment, none of the before-mentioned globules, though divided into a thouſand parts, could paſs through them. I do not here take any notice of thoſe blood-veſſels as large or larger than a hair of one's head, which in great numbers run among the medullary parts of the Brain, and are every where again divided into branches,

When I came to what is called the medullary ſubſtance of the Brain, I obſerved ſeveral irregular globules of different ſizes, ſome of which were equal to the ſize of a globule of human blood, and ſome larger, and which, as well as I could judge by my eye, for the moſt part conſiſted of a thin, tranſparent, and oily ſubſtance. Theſe globules are formed in the medullary part of the Brain (and principally in that part where the ſpinal marrow begins), in ſuch quantities that they ſeem to conſtitute the greateſt part of the Brain. This great multitude of tranſparent globules cauſes the white colour,

becaufe all tranfparent particles, which are not fo connected together that the light can pafs through them in a ftraight line, and do not conftitute as it were an uniform body, muft exhibit a white appearance to the eye, as I have fhewn in * another place. Thefe irregular globules were fo clofely joined, that when I endeavoured to feparate them, fome of them ftretched out, and became twice as long as broad, and they appeared to me joined in the manner of net-work: upon obferving which I began to confider whether the fmalleft blood-veffels, iffuing out of the corticle, might not be again divided into fmaller branches, and many of them furround thefe globules, or give way to them, as we see the small † horizontal veffels in fome kind of wood give way to the perpendicular veffels, and take a circuit round the half of their circumference. This opinion of mine, that many of thefe irregular globules are furrounded by fmall veffels, was confirmed, when I faw many of the faid veffels bright in the middle, and bearing fomething of a dark colour at the fides; and ftill more, when, upon breaking the globules, I found many fmall fibrous parts which feemed to be veffels lying among them. I alfo often faw parts of the medullary fubftance of the Brain of the fame figure, as if we were to behold a fifherman's net, the threads of which can eafily be extended any way, and that between each fpace of the net (which fifhermen call a mefh), a very flexible body, in fhape of a ball, was placed, which would affume either a round or an oblong form, as the net was drawn one way or the other. And to fuch a net I compare the multitude of minute veffels in the medullary fubftance of the Brain, and the balls to the irregular globules I have mentioned. The other parts of the medullary fubftance confifted of an almoft infinite number of exceffively minute globules, and a clear thin matter, which laft I conjectured had iffued from the wounded veffels, and alfo that fome of the veffels themfelves might have been diffolved into it.

* See Vol. I. p. 184, Addition by the Tranflator.

† This may be feen in Vol. I. Plate I. *fig.* 1, at the Letters E E E. *Tranflator.*

Proceeding farther in my inquiries, I took the head of a Sheep, and upon examining its Brain, I alſo found in the corticle, a great number of wonderfully minute blood-veſſels, in which I could diſcover the ſubſtance which gives the blood its red colour; theſe veſſels alſo gave the corticle a darkiſh complexion. I often contemplated this incomprehenſible multitude of exquiſitely minute blood-veſſels, merely for the pleaſure I took in viewing them. For I ſaw almoſt all of them again divide into branches; and I moreover took notice of a great number of globules, each about a ſixth part the ſize of a globule of human blood, and which, I judged, iſſued from the veſſels which were broken; and I concluded, that ſix of thoſe globules would join to make up one globule of blood when they paſſed into larger veſſels, for they were very inferior in tranſparency to the other globules adjoining. I alſo imagined that theſe globules, of which ſix would make up an ordinary ſized globule, when they came to veſſels ſo ſmall as not to admit them, muſt be again divided into ſtill ſmaller parts, and then thoſe veſſels would become colourleſs. And, in order more clearly to give the reader ſome idea of the incomprehenſible ſlenderneſs of the blood veſſels which are formed in the brain, I have made a computation of their ſize, judging by the eye, and by compariſon with a large grain of ſand; I take it then for a certain poſition that thoſe globules which tinge our blood with a red colour are, when perfect, of ſuch a ſize, that one hundred of them laid ſide by ſide, do not more than equal the thickneſs or axis of a grain of ſand, and conſequently, that a million of globules of blood are equal to a large grain of ſand. I next obſerve the blood-veſſels in the Brain, reſpecting which I am certain, that if one globule of blood was divided into ſixty-four parts, none of ſuch parts could paſs through one of thoſe veſſels; for I ſtate the diameter of one of thoſe veſſels to be as 1, and

$$\begin{array}{r} 100 \\ 100 \\ 100 \\ \hline 1,000,000 \\ 64 \\ \hline 64,000,000 \\ \hline \end{array}$$

4 the axis of a globule of blood to be as 4 ; the reſult is, that if
4 a grain of ſand was divided into ſixty-four millions of parts, not
— 16 one of thoſe parts could enter the orifices of the ſmalleſt veſ-
4 ſels in the Brain, and eſpecially, if the ſmall particles of the
— 64 blood were not flexible ; but I think that the ſmall particles
— of the blood are equally flexible with thoſe globules which are of full ſize. I alſo found the corticle of the Brain in the Sheep to conſiſt of a clear vitreous ſubſtance, the ſame as I have deſcribed in the Brain of a Turkey ; with this only difference, that I ſaw in this ſome thin white ſtreaks, which could not eaſily be diſcovered by the naked eye, ſome thinner than a hair of one's head : theſe ſtreaks I judged to be cauſed by a greater collection than uſual, of thoſe large tranſparent globules which cauſe the medullary parts to appear white. I alſo obſerved in the medullary parts ſome dark ſtreaks of the colour of the corticle, which dark ſtreaks, I judged proceeded only from this, that there were few or none of thoſe pellucid globules in that part.

After this, I examined the white or medullary parts of the Brain, and principally thoſe which are the beginning of the ſpinal marrow ; and here I think I ſaw, ſometimes very clearly, what I had not been able to diſtinguiſh in the Brains of Turkeys, namely, that thoſe large and tranſlucid (as they appeared to the ſight) oily globules, were in a manner ſurrounded, or lay in the midſt of, an inexpreſſible number of wonderfully minute veſſels or ſtreaks in the form of net-work, mixed with ſome larger ones, which were in a ſtraight direction and very tranſparent, cauſing the Brain where they were collected in any quantity, to appear uncommonly white to the eye : theſe, by ſeveral obſervations, appeared to me to be about the twenty-fifth part the thickneſs of an hair, and to be of a dark colour. The remainder of the medullary ſubſtance conſiſted of the ſame kind of parts, as I have ſaid I obſerved in the Brains of Turkeys. And yet I ſometimes had my doubts, whether all I deemed to be veſſels were ſo in reality.

After this, I took the Brain of an Ox immediately after the head was cut off, and forthwith proceeded to examine the corticle of the Brain with all the care and attention I was able. In ſeveral examinations of which, I thought I was aſſured that the vitreous and very pellucid ſubſtance (which conſtitutes the greateſt part of the corticle), conſiſted of nothing elſe than of exceſſively ſlender ſtreaks or veſſels cloſely joined together; though at another time, I could not ſatisfy myſelf ſo fully in that reſpect as I wiſhed to have done. With regard to the component parts of the corticle, I could not diſcern any difference from what I have before related of the other ſubjects I examined, except that I did not here perceive ſo much fluid matter, as where the animals had been killed ſome time. The white, or medullary ſubſtance of the brain, I found to be of the ſame nature as I have deſcribed in a Sheep. For, when I obſerved the very white lines or ſtreaks in the medullary part (from whence the ſpinal marrow takes its origin), I found the reaſon of its great whiteneſs, to be a number of very tranſparent veſſels adjoing to each other, which ſeemed to me, formed for conveying from the ſubſtance to nouriſh and ſupport the ſpinal marrow, and in part the nerves: the largeſt of theſe veſſels deſtined for conveying ſuch nouriſhment, ſeemed to me, as nearly as I could judge by my eye, to be about the hundredth part of the thickneſs of a hair of my beard, though at another time, I ſaw them much larger; but in this inſtance, I think I happened on that part of the Brain where thoſe tranſparent veſſels were the ſmalleſt of all.

In all theſe my obſervations, it muſt be underſtood, that I take no notice of the multitudes of blood veſſels diſperſed throughout the Brain, which, upon diſſecting it, may be ſeen with the naked eye. For theſe are, as it were, entire rivers, when compared with the ſmall veſſels I diſcovered, and have been now deſcribing, which may be deemed as the ſmalleſt brooks, channels, and ditches.

I alſo examined the corticle of the Brain in Sparrows, immediately after having killed them. In theſe, I not only obſerved with

equal diſtinctneſs, the ſmall veſſels of every deſcription, as I had ſeen them in the Brain of a Turkey; but alſo as clearly and diſtinctly as I had done in the Brain of an Ox and a Sheep, and in all other reſpects, there was no difference to be noted than between the magnitude of a Sparrow's Brain and that of an Ox; ſo that as before obſerved, there is no difference between them (except in the large blood veſſels), ſave that the ſize in the larger animal is owing to its being compoſed of a greater number of parts. And when, with my utmoſt attention, I examined the vitreous ſubſtance (otherwiſe called the corticle) of the Brain, it ſeems to me wholly to conſiſt of no other parts, than an incomprehenſible multitude of the minuteſt veſſels, which adjoin ſo cloſely to each other, that by their tranſparency they repreſent a glaſſy ſubſtance, when viewed by the microſcope, and which appeared more plainly to me in the ſmall Brain of the Sparrow, than in the large one of the Ox.

Now, though I am certain of the truth of my opinion, nor have any ſcruple or doubt reſpecting the exiſtence of the minute veſſels I have been deſcribing, I had rather it were doubted of by others, if perhaps my ſight has deceived me, but which I ſee no reaſon to believe. For we ſhall ceaſe to doubt or wonder at the minuteneſs of theſe veſſels, if we recollect and duly conſider the minuteneſs of thoſe which the ſmalleſt animalcules we view muſt be furniſhed with; or what will ſeem leſs wonderful, if we conſider what I am going to relate, reſpecting the ſmalleſt veſſels in our own bodies. For being lately buſied in diſſecting a human eye; in the black membrane which covers the cavity of the eye, and by which the rays of light are reflected, I ſaw ſtreaks or veſſels the ſmalleſt that I had ever diſcovered; and being deſirous to judge of their ſlenderneſs, I took a large grain of ſand, the axis of which was almoſt the thirtieth part of an inch: viewing this grain of ſand by the microſcope, I judged that its axis might be divided at leaſt into 330 equal parts; and finding that eight of thoſe ſmall veſſels, placed

2,640
2,640
105,600
1,584,0
5,280
6,969,600
2,640
278,784,000
4,181,760,0
13,939,200
18,399,744,000

ſide by ſide, would not equal $\frac{1}{330}$th part of the axis of the grain of ſand, it follows that the axis of ſuch a grain is 2,640 times larger than the breadth of one of thoſe ſmall veſſels. This number 2,640 being multiplied into itſelf, to find the ſolid contents of a body whoſe axis is 2,640, the reſult is more than eighteen thouſand millions: ſo that a large grain of ſand, as before-mentioned, muſt be divided into ſo many millions of parts before any one of theſe parts can paſs into the cavity of thoſe minute veſſels. And the minute veſſels, which in part conſtitute the vitreous ſubſtance of the Brain, I take to be ſo ſmall, that no particle or grain of large ſand could paſs through them, unleſs it were firſt divided into many millions of parts.

With regard to the Brain of a Sparrow, I found, by many obſervations, that thoſe parts which are called the medullary ſubſtance do, for the moſt part, conſiſt of wonderfully minute fibres or veſſels: and in this part I met with as many ſmall blood-veſſels as I have before mentioned to have ſeen in the corticle. The remainder of the white ſubſtance in the Sparrow's Brain did not differ from that in the Ox, Sheep, and Turkey, as I have before mentioned, except that the tranſparent oily globules, which were encloſed by ſome of the veſſels, were not ſo large in the Ox. But in the Brain of a Sparrow which had been killed twenty-four hours, I found thoſe oily globules, as far as my eye could judge, to be of the ſame ſize as in the brain of an Ox or other animal. From theſe laſt obſervations, and from my former ones, I began to conſider whether that great number of lucid globules (which may be conſidered to be of an oily ſubſtance) were not a fluid in the veſſels, or perhaps the veſſels themſelves, which, while the animal was living, continually conveyed a ſubſtance for the nutriment of the ſpinal mar-

row and the nerves; and that, upon the animal's death, and the motion of the fluids in the veſſels ceaſing, and the parts growing cold, they collected or coagulated into irregular globules of different ſizes, in like manner as we ſee that two or three globules of blood, lying ſo near as to touch each other, do, upon the approach of cold, concrete together and form an irregular figure. And as often as I revolve in my mind the ſituation and figure of the medullary ſubſtance of the Brain, it ſeems to be deſigned by Nature to convey the blood, which in theſe innumerable ſmalleſt veſſels, paſſing through the vitreous part of the Brain (called the corticle), is prepared and elaborated, to the ſpinal marrow: ſo that moſt of the minute arteries which are found in the Brain, ſeem made only to convey the blood to the Brain, without its being returned from thence into the veins; and for this intent only, namely, that the Brain may be continually ſupplied with a freſh addition of blood, and may provide a conſtant freſh ſupply of ſubſtance, for the ſupport and nouriſhment of the ſpinal marrow and the nerves. And as to the objection which ſome may make, that if the blood did really paſs through the Brain, it would tinge the medullary ſubſtance of a red colour; ſuch objection will be found of no force, if we conſider that a * globule of blood, ſingly taken, exhibits very little of a red appearance; and, therefore, when divided into its moſt minute component particles, it may appear wholly colourleſs.

Addition, by the Translator.

The Tranſlator begs leave to ſubjoin a few words to this Eſſay, for the information of ſuch of his readers as are not converſant in the doctrine of ſolids; and the rather, as what the author ſtates reſpecting the minuteneſs of the veſſels in the retina of the human eye may appear, at firſt ſight, utterly incredible. But it muſt be remembered, that it is the ſolid contents of two ſpheres or globes of the ſizes mentioned by the author, and not their reſpective diameters, that are the meaſure of his computation.

* See this explained in Vol. I. page 94.

To illuftrate this, we muft underftand, that if a grain of fand were divided into parts (fuppofing all to be of a fpherical fhape) each of which was $\frac{1}{2640}$th part of the diameter of that grain, each of fuch parts would be capable of entering the cavities of the fmall veffels he is treating of: though, at the fame time, the rules of arithmetic teach us, that the folid contents of that grain would be to the folid contents of one of thofe component parts, as 18,392,744,000 to 1. Again, he tell us, that a globule of blood (of which a million are no more than equal to a grain of fand) muft be divided into 64 parts before it could be received into fome of the blood-veffels of the Brain; and that a grain of fand would be equal to 64 millions of fuch parts. But by the fame rule we fhall find, that the diameter of each fuch part is no more than $\frac{1}{400}$th part of the diameter of a grain of fand. After all, it muft be confeffed, that thefe reduced fubdivifions are not to be comprehended in idea, though not fo inconceivably minute as the other proportions feem to intimate.—See a note on this fubject, taken from the Spectator, in the Introduction to the Firft Volume.

On the Seeds found in the Fig and on Strawberries, with the Author's remarks on the great fecundity of Nature in the propagation of plants and animals.

I HAVE, at times, beftowed much labour in fearching after the young plant in the feed of the Fig, but always without fuccefs; the reafon of which I took to be, that when Figs are packed up at the places of their growth, in order to be exported, they are gathered before they are ripe. At length, however, while I was in fearch after foreign mites (that little pernicious infect which finds its way into all kinds of dried provifions, fuch as cheefe, dried fifh and bacon, and alfo dried fruits, as Figs and Raifins) I found a Fig with feveral hundreds of thefe animals in it, but otherwife of good tafte and full grown, feeming to me as if it had been ripe when gathered. I was, therefore, induced to diffect fome of the fmall feeds it contained; and, breaking open the hard outfide fhells of feveral of them, I took out the entire kernels they contained, then ftripping this kernel of the membrane furrounding it, and clearing away fome moifture which covered the plant, I faw a complete young plant, confifting of two leaves, and of the part from which a young tree would in due time have proceeded.

I have caufed a drawing to be made, from the microfcope, of the young plant in one of thefe feeds (of which feeds we know that every Fig contains great numbers), becaufe I have often heard it faid, that eating Figs in great quantities, will breed lice in the ftomach; and that fome perfons (and among them a man of fome eminence) have not fcrupled to affert the fame in writing; which idle tales certainly do no other than excite derifion, for I am well affured that this notion took its rife from no other caufe than the multitude of fmall grains or feeds with which all Figs abound, but which not one man in a thoufand knows to be feeds, and ftill lefs,

that in every one of thofe feeds there is as much perfection as in an entire Fig-tree; and, therefore, it is very probable, that people comparing thefe feeds or grains in Figs, by reafon of their fmallnefs, with lice, firft broached the idea that lice can be bred from Figs.

Plate XIV, *fig.* 23, A B C, reprefents the kernel or internal part, taken out of the hard fhell of one of thofe feeds, which we obferve in great numbers in every Fig. A B is that part of the kernel to which the ftring or ligament by which the feed received its nourifhment was fixed. That part from which the root and trunk of the tree would proceed, is fituated from A towards C; but the leaves of the young plant lie from C towards B. Every one of thefe grains or feeds in a Fig is diftinct from the reft, being as it were furrounded with its own proper membrane; fo that we may be certain that every feed, while in the Fig, is provided with two ftrings or ligaments, deftined for its nourifhment, namely, one that nourifhes the hard fhell, and the other that brings nourifhment to the kernel within the fhell.

At *fig.* 24, D E F G, is feen the young plant taken out of the feed of the Fig; and in this figure, E F G, denote the two leaves; and G D E, that part which would grow to the root and ftem or trunk. But it muft be obferved, that this young plant does not conftitute the whole kernel of this feed, but that the young plant (befides the membrane which furrounds it) is partly enveloped with a certain kind of fubftance, from which it receives fupport and growth, until the root is grown long enough to extract nourifhment from the earth. All the fpace in the figure, between F D G, was filled with this fubftance.

Now, if we attentively confider and obferve the membrane which furrounds this kernel, we fhall not only fee a great number of exceffively minute veffels, but an incredible quantity of dots or cavities in it, fo that, in a word, if it were poffible to dive into all the hidden parts of this feed, nothing would be feen but wonders and

perfections, for no doubt this minute plant poffeffes as many perfections as an entire tree.

When I had fteeped fome of thefe feeds in water for a few hours, and then opened them, I could diftinguifh, as well in that part which would produce a root as in the young leaves, a great number of veffels with their divifions or cells, but as foon as the liquor evaporated, all thofe veffels difappeared.

Fig. 25, H I K exhibits the fame plant as is fhewn in *fig.* 24, with this difference only, that whereas the former was drawn in a pofition to fhew only the fides of the leaves, in this figure the plant was so placed before the microfcope as to fhew the breadth of the leaves.

I am at times called upon to defend the fyftem I have advanced refpecting the propagation of animals (which is often the cafe with many eminent perfons who come to vifit me), and they continually object to what I have delivered refpecting the abundant provifion made by Nature in the firft rudiments of the young of each fpecies, of which few come to perfection, whereas (fay they) Nature does nothing in vain. The anfwer I generally make to this objection is, by obferving what multitudes of feeds are produced by trees, of which the prefent is a ftrong inftance. For, when we fee that a common Fig contains in it between four and five hundred feeds or grains, and that each Fig-tree may every year produce many Figs, and from each perfect feed a whole tree may fpring; and confequently that a fpace of ground planted with Fig-trees may, in one year, produce fo many feeds as would fuffice to fow, not only an entire kingdom, but the whole furface of the earth, and that this is not only the cafe with Figs, but in every kind of tree whatfoever: when, I fay, we continually fee thefe things, I cannot but give it as my opinion (with due deference to better judgment) that we ought not to call in queftion for what purpofe fuch a fuperabundant provifion is made, but that it would become us better with reverence to acknowledge the Omnifcience of the Supreme Being, who has

thought fit to make ſuch a proviſion for the propagation of animals and vegetables, the reaſons for which order of things are to us inſcrutable, and reſpecting which we can only form conjectures.

In eating Strawberries, my attention was taken by thoſe little knobs or riſings we ſee on their ſurface, and I had no doubt that each of them was a ſeed: to ſatisfy myſelf reſpecting this, I took one of the largeſt and ripeſt Strawberries, and picked out ſeveral of the ſeeds; then ſtripping them of their outward covering, I found each of them provided with a ligament by which it had received its growth, I alſo broke open the hard ſhell, and found in each ſeed that ſubſtance we may call a kernel; from this kernel I took off the membrane, and placing the plant before the microſcope, I cauſed a drawing to be made of it, in order to ſhew what numbers of ſeeds we ſwallow in eating only a ſpoonful of Strawberries; for, cutting one of the largeſt Strawberries into four pieces, I counted in one quarter of it, fifty ſeeds, and conſequently, that one Strawberry was covered with two hundred ſeeds; and one of the ſmaller ones I judged to have an hundred and twenty.

Fig. 26, A B C D E, repreſents the plant taken out of the ſeed of a Strawberry; A B C is that part which would grow to a root; and C D E A, are the two leaves which would firſt appear above the ground.

Theſe two leaves are always moſt exactly joined to each other; but as, while I was taking out the plant from the ſeed, the leaves parted a little aſunder, I directed the limner to draw them in that poſition, that each leaf might be the eaſier diſtinguiſhed.

Now, if we recollect that a young Strawberry plant, (for I never heard of Strawberries being propagated by ſowing), does in one year ſend forth various branches (commonly called runners), which in a proper ſoil conſtantly ſtrike good roots, and grow up to per-

fect plants, fo that in the fame year, many plants are produced from a fingle one, all which in the following year bear fruit, and each of them produces many Strawberries, each of which Strawberries has as many feeds as we have feen: if, I fay, we duly confider thefe things, we muft needs be aftonifhed, and wonder in filence, at the multitudes of feeds which are produced by plants.

On the nature of the minute Fibres which compoſe the fleſhy parts of ſeveral Animals, and alſo of Fiſhes and Inſects.

IN my examination of the component Fibres of the fleſh of oxen and cows, I judged that each ſingle Fibre was ſo minute, that fifty of them, placed together, were no more than equal in diameter to one twenty-ſecond part of an inch; but, if we take the twentieth part of an inch, and allow the other two parts for the membranes in which the fleſhy Fibres are incloſed, it will amount to one thouſand
1,000 fleſhy Fibres in the length of an inch, and, conſe-
1,000 quently, a million of ſuch Fibres, wrapped in their
1,000,000 membranes, will be compriſed in the compaſs of a
ſquare inch. In ſome of my obſervations, it appeared to me, that about an hundred of theſe fleſhy Fibres, lying cloſe together with a membrane ſurrounding them, compoſed one ſmall fleſhy muſcle: at another time I ſaw, in an ox's tongue, three ſmall fleſhy muſcles, each of them enveloped in a membrane, lying cloſe together; and, having cut them tranſverſely, I found that the ſpace they occupied was not ſo much as could be covered by a grain of ſand (one hundred of which grains, placed ſide by ſide, make up the length of an inch). Now, if we ſuppoſe that two hundred of theſe fleſhy Fibres, incloſed in their membranes, compoſe a ſmall fleſhy muſcle, five thouſand of thoſe ſmall muscles will go to make up the dimenſions of a ſquare inch. I compared the thickneſs of theſe fleſhy Fibres with the ſize of a hair taken from my wig, and I judged that at leaſt four Fibres of an ox's fleſh, taken near the ribs, were contained within the ſize of one of thoſe hairs, and that a hair of my beard was nine times as large as one of thoſe Fibres. We muſt not imagine that theſe fleſhy Fibres are round, but each of

them affumes a different figure, fo as to lie together and adjoin as clofely as can be imagined. This my computation of the flefhy Fibres (as they are denominated by a certain medical gentleman in this country), I have here fet down in order to fhew their minutenefs, and the rather as the fame medical gentleman fuppofes, that thefe flefhy Fibres are inferted into the orifices of the veins and end in the arteries, and that the circulation of the blood is performed in thefe flefhy Fibres, and he fays that he has feen valves in them; but thefe his obfervations are not taken from the microfcope, but from the judgment of his naked eye, fo that, doubtlefs, he miftook an entire flefhy mufcle for a flefhy Fibre.

Plate XV. *Fig.* 1, is a flefhy Fibre, in which I have often obferved fome foldings or inequalities, like wrinkles, as A B C D, and others lying near them, as at E F G H, and again as at I K L M, and in this manner, viewed by a common microfcope, they had the appearance of being compofed of globules; but a flefhy Fibre alfo appeared as at N O P Q, and this laft appearance, I think, reprefents truly the internal original filaments of which a flefhy Fibre is compofed.

Upon feeing this, I was able to fatisfy myfelf as to the reafon why our fingers, arms, legs, and our whole bodies, while at reft, do not lie in a right line, but fomewhat bent, and, as it were, tending to the pofition they were placed in before we were born; and, moreover, I think I can affign the reafons of the motions of our limbs, or rather the extenfions and contractions of our mufcles, namely, that when a mufcle is extended the flefhy Fibres compofing it have no wrinkles or folds, but when a mufcle is not extended, but contracted or thicker in figure, then each flefhy Fibre is full of wrinkles or folds.

Fig. 2, A B C D E F G H is a flefhy Fibre which, by expanding and compreffing it, in order more plainly to difcover the number of filaments of which fo fmall a Fibre confifts, I caufed to open very much in its component filaments, and it gave the reprefentation of

a vein with its ramifications: this, to the beſt of my ability, I repreſented in the drawing. Hence it plainly appears, even more clearly than I can expreſs, that the fleſhy Fibres are again compoſed of a great number of filaments. And at one time I ſaw a fleſhy Fibre, ſo accurately expanded and divided, that, in a ſpace a little broader than from B to H in the figure, I ſaw ſeventy of theſe component filaments lying together; whence I concluded, that a fleſhy Fibre no thicker (as I have before-mentioned) than the ninth part of a hair of my beard, contained in it an hundred filaments. I often imagined that I could diſcern the Fibres, or, more properly ſpeaking, the veſſels, of which the membranes in which theſe fleſhy Fibres are incloſed conſiſt: hence I gave farther ſcope to my imagination, and I reaſoned thus: ſince we ſee that a large muſcle conſiſts of ſo many thouſand ſmaller ones, or Fibres, each incloſed within its proper membrane, and that every one of thoſe ſmall fleſhy Fibres is again compoſed of ſtill ſmaller filaments, of which its internal ſtructure conſiſts, each of theſe filaments (one hundred of which go to making up a ſingle Fibre) muſt be alſo a fleſhy muſcle, and may contain within it many ſtill ſmaller filaments, incloſed in a diſtinct membrane. For we ſee that the power and wiſdom of the Almighty Creator of the Univerſe, diſplayed in the formation and aſſociation of the different parts which compoſe his creatures, are ſo wonderful and incomprehenſible, that the deeper we dive into the ſecrets of his created works, the more we are confounded and loſt: eſpecially when we ſee living creatures moving in the water of the ſimilitude of minute eels, as pictured at *fig.* 3, which yet, with all their component parts, are ſmaller than one of the filaments of which a fleſhy Fibre is compoſed; and, nevertheleſs, ſo minute an animalcule muſt neceſſarily be furniſhed with a ſkin, nerves, muſcles, and other parts, all equally perfect as thoſe in a large animal.

If any admirers of the ſecrets of Nature ſhould deſire to follow me in theſe my obſervations, I would adviſe them not to chuſe for that purpoſe a hot and dry ſeaſon, but rather a time when the air is

cool and moift; for when thefe fmall Fibres are divefted of their membranes, and feparated or fplit open, to difcover their component filaments, this operation muft be performed as expeditioufly as poffible (and even then it will often fail of fuccefs); for the moisture of thefe flefhy Fibres will, by reafon of their thinnefs, quickly evaporate; and if that happens, the filaments of which the Fibre confifts will dry up and cohere fo clofely, as to exhibit the appearance of a fingle, uniform, and folid bright fubftance.

In my lateft obfervations refpecting the mufcle of a Hare, I plainly faw how fome of the flefhy Fibres, which were very fharp at the extremities, terminated in the membrane of a mufcle; fome again, at the extremities, terminated in a mufcular tendon. This mufcle I placed before a good microfcope, in order to fhew the fame to a gentleman who was vifiting me; but the mufcle being very flender, the moifture of it foon evaporated, whence, in a very fhort time, the flefhy Fibres became fo clofely joined, that I could not diftinctly fee any one of them feparately, much lefs could I difcover the place where they were united to the tendon, or indeed the tendon itfelf.

Thefe difcoveries induced me to repeat my former inquiries into the nature of thofe Fibres of which Fifhes are compofed; and, upon examining feveral parts of the cod-fifh, I judged that the moft folid fifhy Fibres were in that part of the belly near the gills. Thefe Fibres being divefted of their membranes, I faw in them the fame turnings, foldings, and wrinkles, as I have before noted refpecting the Fibres of flefh; but they had not always the fame figure; for fometimes they appeared as *fig.* 4, B E C D; at another time, as at F G; and alfo as at H I. Upon cutting thefe Fibres tranfverfely, I very plainly faw the ends of thofe multitudes of filaments of which the infide of a fifhy Fibre is compofed. I alfo, but very rarely, faw, on the tranfverfe cutting of thefe fifhy Fibres, certain very bright lines or ftreaks, as at A. I debated in my mind, whether thefe were veffels deftined for the nourifhment of thofe

internal filaments or membranes, to incloſe them; but becauſe I very rarely had a ſight of this appearance, I cannot ſay much reſpecting it. Theſe fiſhy Fibres are of very different ſizes, ſo that I have ſeen ſome four times as large as others; and in one half the circumference of ſuch a fiſhy Fibre, I was well aſſured, I could count almoſt an hundred filaments; and ſome of theſe filaments appeared to my eyes to be divided in their longitudinal poſition; as is in ſome manner repreſented between K and L; ſo that in the circumference of one fiſhy Fibre there were two hundred filaments. And becauſe this number is great, even to admiration, I cannot omit to make a computation of it.

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\begin{array}{r}
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200 \\
\hline
40{,}000 \\
7 \\
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88)\ 280{,}000(3{,}181 \\
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$$

To find the contents of a circle from its known circumference, the rule of Archimedes is, as 88 is to 7, ſo is the ſquare of the circumference of the circle to its contents. And, following this rule, we ſhall find the contents to be 3,181 filaments in a ſingle fiſhy Fibre.

Now, who can in imagination conceive ſuch an incomprehenſible number of filaments in one of thoſe Fibres compoſing the ſubſtance of a fiſh? And who can tell whether each one of theſe filaments may not be incloſed in its proper membrane, and contain within it an incredible number of ſtill ſmaller filaments?

I have alſo examined the formation of the Fibres, as well in the bodies as in the claws of crabs, and I found them like thoſe in the cod-fiſh, compoſed of many ſmaller filaments, and with the like folds or wrinkles as I before noted. Afterwards I examined the Fibres of a ſhrimp, and found their formation to be ſimilar.

Upon diſſecting the hind leg of a frog, I found that the fleſh of it was, in like manner, compoſed of threads or Fibres, which Fibres being divided aſunder, I perceived that each of them was compoſed of a great number of ſmall filaments. But becauſe the circular folds or wrinkles in each of theſe Fibres were larger than

thoſe I had ſeen in the component fleſhy Fibres of other animals, I cauſed a portion of one of them to be drawn, as ſeen through the microſcope: this is repreſented at *fig.* 5, A B C D. The like folds or wrinkles I afterwards met with in the Fibres compoſing the ſmall muſcles in a lamb, taken out of the belly, near the hind leg.

Theſe folds or wrinkles in the fleſhy Fibres, not only convince me what is the reaſon why our limbs aſſume a bent poſture when the muſcles are at reſt, but we may from hence certainly learn the reaſon why we can remain longer ſitting than in a ſtanding poſture, and why, when ſitting, we do not keep our arms and hands fully extended or hanging down, but are always moving them about; for if they remained long in a ſtate of reſt, one muſcle would be too much extended, and another too much contracted, which would be repugnant to the natural diſpoſition of the muſcles. For the ſame reaſons, when we ſtand for any length of time, we do not remain ſupported on both feet, but firſt raiſe up one foot and then the other, ſo that the fore-part of one foot at a time touches the ground, and then the muſcles of that foot are at reſt.

I was at firſt at a loſs to conceive the reaſon why ſome of the circular folds or wrinkles in the Fibres of fleſh and fiſh, as in the figure E F G H and I K L M, they are repreſented; but when we recollect, that theſe Fibres are not round, becauſe they lie cloſely compreſſed together, then if we ſuppoſe one of thoſe Fibres ſeparated from the adjoining ones to be round, or in a ſhort time to aſſume a round figure, we may eaſily conceive how this comes to paſs: becauſe a fleſhy Fibre is compoſed almoſt one third part of a watery ſubſtance, which being ſoon evaporated, the internal part may be wrinkled or contracted in a bent or irregular form. Let us ſuppoſe *fig.* 6, A B C D E, to repreſent a fleſhy Fibre, which ſeparated from other Fibres, will be of a round figure, which by the evaporation of its moiſture may loſe its round ſhape and be

indented and bent inwards, as is ſhewn at F; by which means the folds or wrinkles which before were in ſtraight lines will now become crooked, as appears between B and D.

To purſue my obſervations on the Fibres which compoſe the ſubſtance of animals, I endeavoured to inveſtigate the nature of thoſe in a flea, conſidering, that if I could diſcover it in the fleſhy Fibres and their filaments, we may be aſſured, that the ſame kind of formation prevails in all living creatures. And in this inquiry, I not once or twice, but very often, moſt clearly could ſee the fleſhy Fibres in the breaſt of a flea, in that part where the feet are joined to it; and I ſaw alſo in them the ſame circular folds or wrinkles as I have deſcribed to be in fleſhy and fiſhy Fibres. Some of theſe Fibres were thicker in the middle than at the extremities, as is repreſented at *fig.* 7, A B C D E F G H, which is a fragment of a fleſhy Fibre taken out of the breaſt of a flea. Seeing this, I thought that the fleſhy Fibres in this minute animal, terminated in a point at the extremities, and were there united to the membrane or tendon of the muſcle, as I have mentioned reſpecting the fleſhy Fibres in an ox: and I alſo ſaw ſome of the folds or wrinkles like C F, but for the moſt part they were as at A B G H, and, at various times, I thought I ſaw that theſe Fibres were compoſed of many ſmaller Fibres or filaments.

I continued my obſervations, by examining the fleſh taken out of the feet of a flea, and I ſaw no difference between the formation and figure of the Fibres taken out of the breaſt and the feet, and I ſaw more than twelve of ſuch Fibres in the foot of a flea joining to each other, and alſo many ſmaller Fibres in which I could not diſtinguiſh the folds or wrinkles: theſe laſt I took to be exceeding ſmall blood-veſſels and nerves.

I alſo took the fleſh out of the feet of ſmall flies and ſaw the fleſhy Fibres in them to be formed in the ſame manner as beforementioned.

The Fibres which compoſe the ſubſtance of a whale, I alſo

found to be each inclofed in a membrane, and to be compofed of ftill fmaller filaments; and with regard to the fize of thefe fifhy Fibres, each fingle Fibre was no larger than in the fmaller fifh; and indeed I have feen the Fibres in fome cod-fifh, eight times the fize of thofe in a whale.

I alfo examined the component Fibres in the flefh of a moufe, a calf, and a hog, and found their formation to be the fame as before defcribed, namely, each furrounded with a particular membrane, and compofed of fmaller filaments: the Fibres in the flefh of all thefe animals was nearly of the fize I have before laid down, fo that I may fay, the flefhy Fibres compofing the body of an ox, are not, fingly taken, larger than thofe which go to the fubftance of a moufe, though, as I have computed, the one animal is thirty thoufand times the fize of the other.

On the Scales which cover the ſurface of the ſkin of the human body, and on the formation of the Bones.

THE external ſurface of our bodies is covered with minute particles, which may properly be denominated Scales, placed in regular order beſide each other, but ſo minute that two hundred, or two hundred and fifty of them may be covered by a common grain of ſand, and, viewed by an ordinary microſcope, they appear as in Plate XV. *fig.* 8, H. Many may wonder at the denomination of Scales, and conſider that word as only applicable to the covering of fiſhes. But I ſee no reaſon why we ſhould not aſſign that name to thoſe I am now deſcribing, as well as to the external covering of a trout or a carp, or even of a ſmelt, although theſe laſt are more than an hundred times leſs than thoſe of a trout or carp ; in like manner thoſe particles covering our own bodies, though ſome thouſand times leſs than a ſmelt's, may properly be denominated Scales, ſince they are ſuch in fact, and anſwer the ſame purpoſe on our bodies as Scales do on thoſe of fiſhes. Theſe Scales on our bodies, as I have before ſaid, lie in exact order beſide each other, in like manner as in fiſhes ; and I not only could ſee that their ſhape was a figure of five ſides, but in many I could perceive parallel borders or ridges, which I imagined denoted the growth or increaſe of each Scale, as by a microſcope we obſerve in fiſhes. A drawing of one of theſe Scales, as viewed by the microſcope, is given at *fig.* 9, K ; and this I judged to be a perfect Scale, which had been fixed in the ſkin at the end O P, and this part I always found to be not ſo broad as

the upper end. Many other Scales were not ſo long as this, nor their ſides ſo ſmooth, and theſe I judged I had pulled or broken off the ſkin before they had come to their perfect ſize and growth. Theſe Scales are wonderfully thin, for I judged them to be twenty-five times broader than thick; and I alſo perceived, that they were placed in triple rows on each other; or, in other words, that our ſkin is covered with three layers of them, for only one third part of each Scale is viſible to the eye, and under it are, at leaſt, two other Scales, which are covered by it, as appears at *fig.* 11, M, the part of which marked 1, 2, 3, 4, is only viſible, the remainder being covered by other Scales. That part of the Scale marked 3, I ſeldom perceived ſo ſharp pointed as here pictured; but as I conſidered this to be one of the moſt perfectly formed Scales I had met with, I copied its figure in the drawing. In fiſhes we alſo ſee, that while their Scales are fixed to their bodies, only a part of them is viſible to us. But whereas, fiſhes never caſt their Scales, at leaſt as I could ever obſerve, this is not the caſe with our bodies, for I myſelf have ſeen on my own body a thouſand Scales and upwards ſeparated from the ſkin and adhering together; but when I pulled off any Scales ſticking to my ſkin, and which I conceived were newly formed, either an effuſion of blood followed, or a red ſpot appeared. I alſo took ſome Scales from the ſkin of my arm, in a place where a ſcar had been left on my being let blood about twenty-five years ago, and I ſaw many of them ſhaped as at *fig.* 10, L.

I obſerved on many of theſe Scales certain tranſparent irregular ſtreaks, which I have repreſented in *fig.* 11, M, and theſe covered here and there with round globules about a ſixth part the ſize of a globule of blood, and theſe are alſo repreſented in the figure; and theſe, I ſuppoſed to be occaſioned by exſudations from the body left upon the Scales. From this conſtruction of our external ſkin, I think, we may conclude that fleas, lice, and other inſects, do not

need much force to inſert their ſtings, becauſe they can introduce them between the rows of Scales, and not ſtrike through the ſolid ſubſtance of the Scales.

Theſe my obſervations, ſeemed to me to prove, that there are no particular pores in the external ſurface of our ſkins, but that the moiſture which proceeds from our bodies (particularly when we perſpire), may, at the ſame time, iſſue in many places between each Scale, though the Scales appear firmly united to each other; and there may be between each of the Scales ſmall canals or ducts through which the moiſture may find a paſſage. Now, if we recollect how many places there are which produce the Scales, and that in a ſpace covered by one Scale, there may be an hundred places through which the perſpirable matter iſſues; and again, that two hundred Scales, regularly diſpoſed, may be covered by a common grain of ſand, we may conclude, that there may be twenty thouſand paſſages in our bodies for perſpiration, in a ſpace covered by a grain of ſand, excepting only the places wherein the Scales are rooted, or from whence they grow. Hence it appears, that our bodies may be, as it were, one ſingle pore throughout, while phyſicians are always talking of the pores of the body, as if there were in it particular holes or orifices for that purpoſe, and eſpecially where they ſee a ſmall round drop of ſweat, not conſidering the effect of the air, and that a ſmall portion of moiſture, iſſuing from many thouſand apertures, when preſſed by the air, muſt form itſelf into a globule.

I have ſeen a drawing, made by a gentleman of ſome eminence, repreſenting certain furrows in the inſide of the fingers and the hand, and publiſhed to the world as indicating the pores of the ſkin; but, for my part, I never conſidered theſe furrows, either in the hands or feet, as containing particular pores or orifices in the ſkin, and I always perceived a leſs propulſion of particles from theſe furrows than from the places round about them; and, upon examination, we

ſhall find the ſcales which cover the ſkin placed more cloſely together in thoſe furrows than on the more elevated parts. And, for ſome time paſt, it has been my firm opinion, that the furrows or wrinkles on the inſides of our hands and feet, are only thoſe places where the external ſkin, by means of ſome ſtraight veſſels in it, is uncommonly cloſe and compact, as we ſee in ſome trees which have furrows or wrinkles in their bark, proceeding from the veſſels of the wood, which furrows are not made to emit any moiſture from the wood, but only, as I imagine, to unite the bark more cloſely to the wood. For, as our external ſkin is only made to defend the parts within it, ſo the bark of a tree is appointed for the defence of the wood; and as our external ſkin, when a new one is formed under it, peels off by pieces, ſo the bark of a tree, when the new bark is formed, every year breaks off, piece by piece, unleſs the old and new bark cohere too ſtrongly: and, in like manner as in our hands and feet there is a ſtrong coheſion of the old and newly formed ſkins, it neceſſarily follows, that, in the place where the coheſion is ſtrongeſt, there muſt be wrinkles or furrows in the ſkin.

I examined the Scales on the inſide of my hand, and taken from that thick ſkin we call a callus, and I found the Scales to be of the ſame dimenſions with thoſe on the other parts of my body, but whereas thoſe are thin and tranſparent, theſe were covered with ſo many globules and ſtreaks, that they appeared compoſed of globules. But, as I have always found that not only the inſides of the fingers but all the inſide of the hand emits much moiſture, I conclude, that as the Scales on our arms and other parts of our bodies, when they have no nouriſhment fall off, ſo, on the contrary, though the Scales on the inſide of our hands and feet may want nouriſhment, they collect together in great numbers, and, by the moiſture of the parts, are ſo cloſely glued or coagulated together, and ſuch ſmall ſpaces left for the tranſpiration, that what appears a thick ſkin is formed; though in reality it is, for the moſt part, no more than a collection of Scales;

and this is ſtill more increaſed by hard manual labour, which increaſes the collection and coheſion of particles.

After this I examined the outward ſkin of my mouth, particularly my under-lip, and found this to be alſo covered with Scales, but ſomewhat larger, broader, and thinner than thoſe of the reſt of our bodies. *Fig.* 12, is a Scale of this deſcription, being one of the moſt perfect I met with; theſe Scales were alſo covered with a greater number of ſtreaks; produced, as I ſuppoſe, in the manner I have mentioned above; and likewiſe with many very tranſparent globules, which are pictured in the figure. Another of theſe Scales, adjoining to the former, was of the ſhape repreſented at *fig.* 13. But theſe Scales were not placed as they are on other parts of our bodies, that is, three at leaſt on each other, which is the reaſon that our ſkins appear white, (for all tranſparent particles, if laid looſe on each other, exhibit that colour; ſuch as paper, froth, ſpittle, pounded glaſs, ſnow, and the like); but, on the contrary, the Scales on the lips, as far as I have been able to diſcover, only cover each other lightly on the edges, ſo that the fleſh and the redneſs of the blood are ſeen through the greateſt part of each Scale, and for this reaſon the inſide of the lips and mouth are red.

After this, being deſirous to inveſtigate the manner of the formation of the Scales on our bodies, I judged that theſe laſt mentioned Scales, taken from the lips, would be the fitteſt for that purpoſe, becauſe they are more eaſily ſeparated from the ſkin before they become dried, than on the other parts of the body. And, after repeated obſervations, I ſaw that almoſt all of them had in the middle a bright ſpot, riſing above the reſt of the Scale; which ſpot I had formerly thought was cauſed by accident; and from hence I now concluded for a certainty, that not only the Scales on our lips, but alſo on our whole bodies, are formed, in like manner as the Scales of fiſhes, of veſſels, by which they are increaſed in

ſize; and if this is ſo, what ſhall we ſay of the perſpirable parts of our bodies? I have already mentioned, that perſpiration may be performed through the interſtices between the Scales, which muſt be in great numbers; but the veſſels compoſing the Scales will much exceed that number; for if theſe Scales are in conſtant growth, by the means of ſuch veſſels, then, by the continual motion of the body and of the thin juices, the perſpirable matter may be expelled in great quantities through theſe veſſels.

And, to make a computation of the almoſt infinite number of theſe minute veſſels, I placed ſome of theſe Scales, taken from the ſurface of the ſkin, near ſome grains of ſand, and I judged that the diameter of theſe grains was from ten to twenty times the diameter of thoſe Scales; and the Scales are, as before mentioned, placed in a triple row one over another. I will take, therefore, the ſmalleſt number, and ſay, as I have before done, that
250 250 Scales can be covered by a grain of ſand. And
500 ſuppoſing each Scale to be compoſed of 500 veſſels,
125,000 then the perſpirable matter may be expelled through
125,000 paſſages, within the compaſs of a grain of ſand, beſides the orifices or outlets it may find between the Scales.

With a ſharp-pointed inſtrument I took from the arm of a Negro girl, about thirteen years of age, a ſmall portion of the upper ſkin, and found it to conſiſt of nothing but Scales, joined together as I have ſaid of my own ſkin; but theſe Scales were no ſo large, by reaſon, as I concluded, that the girl was not come to her full growth; for an infant has as many Scales on its body as a grown perſon, and they increaſe with it in ſize. And I am certain, that though a perch, jack, or cod-fiſh, be no bigger than a joint of one's finger, their bodies will have as many Scales on them as when they are grown larger. On placing theſe Scales, taken from the Negro girl, before the microſcope, I found them not ſo tranſparent as thoſe on my own body; and in the place on her arm whence I

had taken them, I ſaw there was left a black ſpot. Theſe Scales I conceived to be ſuch as were almoſt ready to fall off; for when I attempted to take others, which were firmly fixed in the ſkin, an effuſion of blood followed. When I placed two of theſe Scales one on another, they aſſumed a blackiſh colour; and we muſt not wonder, that when viewed ſingly through the microſcope they ſhould be tranſparent, and yet when on the ſkin appear black, for if ink, though very black, is placed very thin before the microſcope, it will only ſeem a little darker than common water; and the ſame is the caſe with reſpect to black ſilks, for if a filament of ſuch ſilk, as thin as what is ſpun by the worm, is viewed through the microſcope, it will appear, not black, but of a dark colour and alſo ſomewhat tranſparent. But many particles, laid one on another, although not wholly black, will appear ſo to our eyes. And hereupon, finding that the blackneſs of Negroes is only cauſed by the Scales being of that colour, I was enabled to form a better judgment than I had before done reſpecting the appearance of children newly born, for they are almoſt always of a red colour, by reaſon that the Scales on their bodies, or only the firſt rudiments of them, are then beginning to appear; ſo that the blood is ſeen through the ſkin, giving it a red colour: and the ſame is the caſe with Negro children newly born, with this difference only, that they are of a deeper crimſon than our children. And I am certain, that as our children, by the growth of tranſparent Scales on their bodies, become white, ſo the children of Negroes, by the growth of the Scales on their bodies become of a black colour. The only thing that puzzled me about this Negro girl's ſkin was, that on the inſide of her hands, and the bottom of her feet, the Scales forming that part called a callus were quite white; but an old lady in this town, who formerly lived in Brazil, and to whom the girl's grandmother had been a ſervant, told me, that this girl's parents were from Angola; and that though all Negroes are born red, and

by degrees acquire a black complexion, yet the infides of their hands and the foles of their feet are always white; whence I concluded, that the Scales on their hands and feet are tranfparent.

I know that there are many men among us who think (and I have heard fome fay fo), that the blacknefs of the Negroes proceed from no other caufe than their bodies being rubbed with fome kind of oil. For, fay they, when born they are of a red colour like our children. But in like manner as it is impoffible to dye the wool or hair on the body of a fheep, horfe, or other animal, fo that it fhall retain the colour, becaufe the hairs or wool are continually falling off; fo it is impoffible to dye thofe Scales that cover the fkin, fo as to remain black, becaufe they very foon drop off. And perhaps alfo the fmall veffels which compofe the Scales on the bodies of Negroes may affume a black colour, whereby the Scales will appear ftill blacker, as we fee the veffels in fome of the coats of the eye are quite black, and I doubt not are fo formed to preferve the blacknefs in the coats they compofe.

Addition, by the Translator.

The perufal of this effay calls to my recollection a paffage in one of Mr. Leeuwenhoek's Letters to the Royal Society, not inferted in his works, but printed in the Philofophical Tranfactions; in which, treating of the callus on the hands and feet, Mr. Leeuwenhoek confirms what he has here laid down, that a callus is only caufed by great numbers of Scales heaped on each other. And he alfo obferves, that on wafhing and afterwards wiping his hands, he was furprized at the great number of Scales which continually came away. Hence we may gather the reafon why, if the hands are ftained with any thing (fuch as the juice of walnut peel) which will not immediately wafh off, the ftain in a fhort time wears away: and why, in a long receffion from labour, the hands become fmooth and foft, forafmuch as the difcoloured and concreted Scales fall off, and are replaced by new ones of the natural colour and confiftence.

I HAVE made many obfervations to difcover the real formation of the Bones, and in my lateft obfervations on the folid part of an ox's thigh Bone, I plainly faw that it confifted of four different kinds of tubes, running lengthways in the Bone, the leaft of which are fo fmall, and lie fo clofe together, that they cannot eafily be diftinguifhed in a Bone cut tranfverfely; and even if the Bone is cut with the fharpeft knife that can be had, nothing will be feen but the appearance of globules; but if the Bone is fplit or cleft, fome fragments will be broken off in which thofe fmall tubes will be perceived.

The next kind of tubes (fome of which are four, fome fix times larger than the former) are likewife not eafily difcovered; becaufe, let the knife we cut them with be ever fo fharp, the hardnefs of the Bone will caufe many pieces to be broken off, which, for the greateft part, will clofe up or conceal the mouths or openings of thofe tubes.

The third fort of tubes, which are much larger than the laft, will become fo ftopped up in the cutting with a knife, that it will be difficult to difcover their cavities; but I have obferved thefe tubes lying difpofed in fuch a manner, that I was well affured a circle of thofe tubes formed every new concretion or addition to the Bone, almoft in the fame manner as I have laid it down in regard to the growth of timber, by the addition of a circle or ring of tubes, formed in the growth of the wood; and efpecially when I faw, that in a fmall fpace from thence, another circle of tubes was to be feen.

The fourth kind of tubes, ftill much larger than the laft preceding, were fewer in number, fo that often, in the fpace of three or four grains of fand, I could not fee one of them.

I have made as exact a drawing as poffible of a fmall particle, taken from an ox's thigh Bone, as the fame appeared to me through the microfcope, and which is fhewn at *fig.* 14, A B C D. This

fragment, to the naked eye, appeared no larger than the ſpot, *fig.* 15. E F G is the point of a ſmall needle to which this fragment of Bone was fixed.

The firſt and ſmalleſt ſort of the tubes I have been deſcribing I could not obſerve in this fragment; becauſe, when the Bone is thus cut, the ſmall tubes appear lying together in a confuſed manner, like irregular globules; the ſecond ſort of tubes I have repreſented in the figure, and theſe often appear like dark ſpots, becauſe their orifices are ſtopped up in cutting the Bone, and are more difficultly diſtinguiſhed when the knife does not divide them accurately by a tranſverſe ſection; for if the ſection is ever ſo little oblique, it is impoſſible to diſcover them. This ſecond ſort of tubes are repreſented at letters H H H.

The third ſort of tubes I have repreſented at I I I: this third ſort I have not only ſeen adjoining each other in circular order, but alſo in a different order, in like manner as the large veſſels in wood.

The fourth ſort of tubes, which are very large in compariſon with the former ones, is pointed out by the letters K K. The curved line L, and that at M, are cracks in the Bone, made by the edge of the knife, which often happens, eſpecially when the knife is not ſharp enough.

Beſides the four ſorts of tubes before mentioned, forming lengthways the ſubſtance of the Bone, I often imagined I ſaw ſome tubes taking a contrary courſe, and which ſeemed to me to proceed from the internal part, and terminate at the ſurface of the Bone; and I alſo thought that theſe were of two ſizes, the leaſt which I alſo imagined, were analogous to the ſmalleſt of thoſe tubes which lay lengthways in the Bone.

The reaſon why I could not truly perceive the tubes proceeding from the cavity to the circumference of the Bone was, I think, this, that theſe tubes were far diſtant from each other; and, indeed, I thought that one tube lay among the longitudinal ones, as if an

opening had been made there for it. And though I could not be quite certain as to my feeing thefe tubes, I do not doubt that there are a great number of them in the Bone; and the rather, as I think it is to be noted, that the membrane covering the Bone is chiefly formed out of thefe veffels, and that it is alfo fupported by them. And of this we cannot doubt, when we obferve, that fome trees have very fmall veffels difperfed among the perpendicular veffels, and by means of which the bark is chiefly compofed.

And in like manner as we cannot with certainty point out the formation of the bark in the tree, becaufe it is formed every year out of the horizontal veffels; fo (according to my hypothefis), we are never likely to draw any other conclufion, than that the membranes covering the bones, receive their growth and nourifhment from certain veffels which proceed from the cavity to the circumference of the bone, and there continuing to grow, are changed into thin and foft veffels, which protect the bone, in like manner as we obferve the bark of trees are formed out of the wood, and defend it from external injury.

I know that many think the origin and nourifhment of the bark of trees depend upon the root; but if it was fo, we fhould obferve thofe parts of the bark next the root to be very thick, and towards the upper part of the tree to grow thinner, and fpread into branches, in like manner as we fee the arteries fpread from the heart, and the nerves from the brain; whereas, in the veffels forming the bark of trees, they are the fame in the upper parts as about the roots, and what is more, the veffels in the bark of divers trees, and efpecially the birch, cherry, peach, and goofeberry, do not take an upward courfe, as in the oak, afh, elm, filbert, apple, and pear trees, but run only to the furface round about the tree; and as the barks of trees whofe veffels mount upwards every year, increafe in thicknefs; for when a tree increafes in thicknefs, then the external part of the bark divides in fiffures, and the old and dead bark adheres to the new; and for this reafon, the older trees are, the thicker their bark, though only

a ſmall part of the bark is alive, and that is what is next to the tree. But the caſe is otherwiſe, with regard to the bark of trees, where the veſſels forming it, take a circular courſe round the tree; for when the trees increaſe in ſize, theſe veſſels cannot ſeparate aſunder, but break; whereupon, the old bark is ſeparated from the new and falls off, for which reaſon, ſuch trees have always a thin bark, and this is plainly to be ſeen in the birch.

And in like manner as it has been ſaid, that the barks of trees are formed and nouriſhed, not from the root, but the wood, and that for the ſame reaſon, the bark is not formed in branchings. The ſame I alſo conſider to be the caſe, in the production and nouriſhment of the real ſkin of our bodies, which is covered by an upper ſkin compoſed of Scales; for having examined the ſkin of ſeveral animals, I obſerved, that in its texture it was not produced irregularly, but I muſt confeſs, that the texture of the true ſkin was, throughout, an entire uniform body; and I alſo imagined, that all the veſſels (excepting the arteries, veins, and nerves), of which our true ſkin is compoſed, do run one among another in a wonderful manner to form a ſkin which ſhall be of extraordinary ſtrength; and that at length they grow thinner and thinner, till at their extremities, they form Scales, with which the ſkin is covered, and then theſe very thin veſſels have no other termination than by being formed into Scales, ſo that each Scale conſiſts of as many veſſels as there were extremities compoſing the Scale, and that each Scale remains united to the veſſels until a new Scale is formed under it.

On the calcareous ſubſtance which is found in the excreſcences on the limbs of gouty perſons, and commonly called Chalk-stones, with the Author's opinion reſpecting the poſſibility of diſſolving the ſalts found therein; and alſo on the cure of the Gout, by burning with the Moxa of the Chineſe.

A Relation of mine being greatly tormented with the gout, and having made an inciſion in the heel of his foot, took out thence a quantity of that ſubſtance which phyſicians call Calx, or Chalk, and ſome of this being put into my hands, I found, upon examination, that it was compoſed of ſmall irregular particles, reſembling a heap of grains of ſand: viewing theſe by the microſcope they appeared of a very dark colour, and each particle to be compoſed of a great number of oblong and tranſparent figures, which cannot better be deſcribed than by a parcel of cuttings of horſe-hair, with each extremity terminating in a point. I judged theſe to be ſo ſlender that many thouſands of them would not be ſo thick as a hair of one's head. I have made a drawing of theſe, in order to ſhew the proportion of their length with their thickneſs, as may be ſeen at *fig.* 16, A, Plate XV. I ſaw many parcels of theſe particles lying in very regular order, as at B, and though I cannot ſay that they were ſo diſpoſed throughout the whole ſubſtance, yet I believe they were originally formed in regular order of two, three, four, or more of them placed together. Theſe ſmall pieces of gouty Chalk I not only ſpread about, but alſo ſeparated ſome of the above minute component parts, and I ſaw ſome of them lie in the poſition deſcribed at B, but many of them without any regular order, as at C. I alſo ſaw ſome of them not above half, or one third part, or a quarter the length repreſented at A, but as repreſented at D; though I do not think this

was originally their length, but that they were the fragments of particles which had been broken in the handling. Among them, alſo, were ſome irregular particles, which I judged to be globules of blood, mixed with the gouty matter.

Being deſirous to purſue this ſubject farther, and to examine whether, by boiling the chalky ſubſtance in water, I could diſcover any acute ſaline particles paſs from it into the water, I procured ſome newly taken out of the finger of a gouty perſon. This Calx was very white, and of a tough and gummy conſiſtence.

Upon ſpreading this upon the ſurface of a new glaſs, I obſerved ſome globular parts lying in a tenacious or gummy kind of ſubſtance; they were of different ſizes, the largest equal to grains of ſand, and others adjoining to them only one twenty-fifth part of that ſize. The viſcous matter was very tranſparent, mixed with many very minute tranſparent globules and oblong ſaline particles, many of which, viewed through a common magnifier, appeared like the fragments of a man's beard of a week's growth, others ten times thinner and one third the length; in a word, of many ſizes, as well in length as thickneſs.

Having ſcattered ſome of this ſubſtance on a very thin piece of glaſs, I placed it before the microſcope, directing the limner to make a drawing of it as it appeared to him, in order to give the reader ſome idea of the nature of this calcareous matter, and which is repreſented at *fig.* 17, I K L M. The whole of the globules here ſhewn, and the reſt of the ſubſtance, were not altogether the ſize of a ſmall grain of ſand. In the figure is alſo to be ſeen the very minute ſaline particles I have mentioned.

I then endeavoured to diſſolve this calcareous ſubſtance in clean rain water, and for that purpoſe put it into a new glaſs, and applied ſo ſtrong a fire to it that the water boiled. I then again examined it, and ſaw the globular parts, which were of different ſizes, and among which ſome of the oblong particles before mentioned could be diſtinguiſhed, adhere as firmly together as if the water had

never been heated. Thefe minute oblong faline particles were fpread in fuch quantities in the water, that I did not think any of them had been diffolved in it.

I let this water ftand for a time, in order to fee whether the particles which gave it a whitifh colour would fink to the bottom; but the water remained turbid, and when I fuffered it to evaporate, there were fo many minute particles left, that the whole fubftance appeared white.

This water, wherein I had fo boiled the calcareous matter, I kept in my clofet during the whole winter, and then again examining it, I faw the thin oblong particles before mentioned (which I judged to be fixed falts) lying in as great abundance as if the Calx had been newly extracted from the perfon's body.

From thefe obfervations, I concluded, that this calcareous fubftance, when in any part of a human body, cannot, by any medicine, be diffolved.

A small quantity of this Calx or Chalk, taken from the fame perfon, I put into a fmall box well clofed, and kept it for fome months in my clofet, I then put it into a new glafs, and gradually expofed it to fo fierce a fire, that the glafs became red hot: I then perceived that, by the force of the fire, a thin pellucid liquor was extracted, and many globules of a yellow colour, but many of them afterwards, uniting together, exhibited a red appearance; but as foon as the cover of the glafs, to which the thin tranfparent liquor and oil adhered, began to cool, all the liquor coagulated in an incredible number of faline particles, moft of which were twenty-five times longer than broad, and were of various fizes, both ends terminating in points, as reprefented at *fig.* 18, A B C.

The oil which by the fire was extracted from this fubftance, and which while warm was a liquid, I faw was now changed into a folid fubftance, which I concluded was certainly occafioned by the multitude of faline particles coagulated in it: thefe particles in the oil were

much ſmaller than the preceding ones, but for the moſt part of the ſame ſhape.

I placed the glaſs to which theſe particles adhered in my cloſet, to ſee whether the particles, when the weather was rainy and the air damp, would not diſſolve into a thin and pellucid liquor, and after twenty-ſix hours had elapſed, I ſaw their figures were all changed, for they were not only become ſhorter but aſſumed an irregular ſhape, nor did I ſee any ſaline particle which was not ſurrounded with ſome tranſparent liquor, and the oil itſelf was become thin and fluid.

After a few days I again inſpected the glaſs, and then I ſaw all the ſaline figures changed into a thin tranſparent liquid, mixed with many ſmall irregular and round particles.

But what appeared principally worthy of notice in theſe my obſervations was, that among ſo many ſaline particles there appeared none of the ſhape of our common ſalt, that is to ſay, four-ſquare, or whoſe baſis was four-ſquare.

Upon the burnt aſhes or caput mortuum of this calcareous matter, I poured ſome fair rain water, that if there were any fixed ſalt in the aſhes, it might be mixed with the water; and having well ſtirred the whole together, and left them to ſtand until the water became clear, I then took a little of the water and expoſed it in my ſtudy to the air, which was then damp, and ſoon after I ſaw in it a great number of thin flat ſaline particles of various ſizes, which, for the moſt part, were of the ſhape repreſented at *fig.* 19, D E F.

I twice touched this water with my tongue, and was ſurpriſed at the great ſaltneſs which ſo ſmall a portion of the aſhes had imparted to the water: but the taste of this ſalt was not like that of common ſalt, this being very aſtringent. I five or ſix times breathed on this fixed ſalt, and then I perceived moſt of the ſaline particles become liquid, but as ſoon as the moiſture of my breath was evaporated, they again concreted in irregular figures.

I ſometimes obſerved, upon ſpreading the water very thin, a great number of thin oblong particles gathered together, of the ſame figures as I had ſeen the minuteſt particles of the calcareous matter when taken out of the perſon's finger; but I thought, if there had been more water, thoſe particles would have been larger and flat.

I often repeated theſe obſervations, to ſee whether I could diſcover any thing in this ſalt bearing an affinity with our common ſalt, but with all my attention, I never could diſcover any particles ſimilar to thoſe of common ſalt.

I again examined the water in which ſome of the calcareous matter had been boiled, and which had remained infuſed in it a whole winter, and though it was thick and turbid, I diſcovered in it an incredible number of minute particles, which all appeared round, nor did I diſcover any other ſaline figures than what are in common rain water, except that at one time I ſaw two ſaline figures in a place where the water had ſtood deeper than the back of a knife, but was now almoſt evaporated: theſe are ſhewn at *fig.* 20, G and H.

Upon the whole, we may conclude, that the ſalt of which the calcareous matter in the limbs of gouty people does for the moſt part conſiſt, is of ſuch a hard and tough ſubſtance, that though it be boiled, or ſuffered to remain in water a long time, the ſaline particles it contains will ſcarcely, if at all, liquefy or be diſſolved: and hence we may conclude, as before mentioned, that it is impoſſible for phyſicians or ſurgeons to diſſolve, diſcuſs, or diſſipate thoſe knots or ſwellings which ariſe in the joints of gouty people, and are commonly called Chalk Stones, by any kind of plaiſter or fomentation; but that the only thing which promiſes any ſucceſs in this way, muſt be by burning the part in the manner practiſed by the Chineſe, who for this purpoſe uſe what they call Moxa.

This is a certain preparation uſed in the Eaſt, and mentioned by

H. Buschoff, a writer on the ſubject, in theſe words:—" Moxa is " a ſoft woolly ſubſtance, prepared with great art, from a certain " dried herb; the name of the herb I know not, but I do not " doubt that it is one of the moſt excellent in all the world. " No druggiſt or apothecary in all Europe knows the manner of " preparing Moxa, but the art of doing it, is held in ſuch eſtima-" tion by the Chineſe and Japaneſe, that they will not ſell the ſe-" cret to other nations at any price."

My opinion upon the ſubject is, that Moxa is only an excreſcence on ſome fruit, like the woolly ſubſtance on peaches, and ſome other fruits; (and *Wilhelm ten Rhyne*, another writer on the ſubject, ſays, that it is the woolly part of a leaf) and I have at times collected divers woolly ſubſtances, which are found on ſome leaves and fruits, and burnt them, particularly that wool which falls from the poplar tree; this, at firſt ſight of it, I hoped would take fire very readily, and I thought the ſame of the wool produced by the willow, about the beginning of June, which is of a more inflammable nature than what is gathered from the poplar. But I do not as yet know of, nor have found any vegetable woolly ſubſtance, which burns with ſuch facility as cotton; therefore, I would recommend it to thoſe who are afflicted with the Gout, and deſire to try the experiment of a cure by fire, and cannot procure Moxa for that purpoſe, to uſe cotton, which may be had in plenty. I have alſo tried the experiment of burning with common tinder made from linen; but this burns ſo fiercely, and penetrates ſo deep, that I believe the uſe of moxa or cotton ten times, would not burn ſo deep as one ſingle application of common tinder.

Addition, by the Translator.

It may not be unacceptable to the reader, to fee what has been faid on this fubject by Sir William Temple, who was cotemporary with our author, and one of the moft eminent perfons of his time. He himfelf tried the application of Moxa in a fit of the Gout, and the following is an extract from his Mifcellanea, in his Effay on the Cure of the Gout by Moxa.

"Being in the feven-and-fortieth year of my age, at the Hague, about "the end of February, one night at fupper, I felt a fudden pain in my "right foot, which, from the firft moment it began, increafed fenfibly. I "went to bed, but it raged fo much all night, that I could not fleep a wink. "I endured it till the next morning, and then making my complaints and "fhewing my foot, they found it very red and angry, and to relieve my ex-"tremity of pain, began to apply common poultices to it, and by the fre-"quent change of them I found fome eafe, and continued this exercife all "that day and a great part of the following night, which I paffed with very "little reft. The morning after, my foot began to fwell, and the violence "of my pain to affuage, though it left fuch a forenefs, that I could hardly "fuffer the clothes on my bed, nor ftir my foot but as it was lifted.

"By this time my illnefs was concluded to be the Gout, and among many "other friends who came to fee me, Monfieur *Zulichem* paid me a vifit, "and in talking of my illnefs, he afked me whether I had ever heard the "Indian way of curing the Gout by *Moxa*. This *Moxa*, he faid, was a "certain kind of mofs that grew in the Eaft Indies; that their way was, "whenever any body fell into a fit of the Gout, to take a fmall quantity of "it, and form it into a figure, broad at bottom as a two-pence, and pointed "at top; to fet the bottom exactly upon the place where the violence of the "pain was fixed, then with a fmall round perfumed match (made likewife "in the Indies), to give fire to the top of the mofs, which burning down "by degrees, came at length to the fkin, and burn it till the mofs was con-"fumed to afhes. That many times the firft burning would remove the "pain, if not, it was to be renewed a fecond, third, and fourth time, till it "went away, and till the perfon found he could fet his foot boldly to the "ground and walk. This operation Monfieur *Zulichem* faid, he became "acquainted with by the relation of feveral who had feen and tried it in the

" Indies; but particularly by an ingenious little book written of it by a " Dutch minifter at Batavia, who being extremely tormented with a fit of " the Gout, an old Indian woman coming to fee him, undertook to cure " him, and did it immediately by this *Moxa*; and after many experiments " of it there, he had written this treatife of it in Dutch, for the ufe of his " countrymen, and fent over a quantity of the mofs and matches to Utrecht, " to be fold, if any could be perfuaded to ufe them.

" I had always heard that a fit of the Gout ufed to have fix weeks at the " leaft for its ordinary period, a delay very diftreffing to one in my fitua- " tion, for I was at that time preft in my journey to *Nimeguen*, by his " Majefty's * commands, to affift at the treaty there. Moft of the ambaf- " fadors from the feveral parts of Chriftendom were upon their way, one of " my colleagues was already upon the place, and I had promifed imme- " diately to follow. This made me ponder on Monfieur *Zulichem's* new " operation; and upon reflecting how many cures have been performed " by fire, I refolved upon making the trial, and difpatched a meffenger to " Utrecht to bring me fome of the Moxa, and learn the exact method of " ufing it from the man that fold it, who was fon to the minifter at Bata- " via. He returned with all that belonged to this cure, having performed " the whole operation upon his hand, by the man's direction. I immedi- " ately made the experiment in the manner before related, fetting the " *Moxa* juft upon the place where the firft violence of my pain began, " which was the joint of the great toe, and where the greateft anger and " forenefs ftill continued, notwithftanding the fwelling of my foot; fo that " I had never yet, in five days, been able to ftir it, but as it was lifted.

" Upon the firft burning, I found the fkin fhrink all round the place; " and whether the greater pain of the fire had taken away the fenfe of a " fmaller or no I could not tell; but I thought it lefs than it was: I burnt " it the fecond time, and upon it obferved the fkin about it to fhrink, and " the fwelling to flat about it more than at firft. I began to move my toe, " which I had not done before, but I found fome remainders of pain. I " burnt it the third time, and obferved ftill the fame effects without, but a " much greater within, for I ftirred the joint feveral times at eafe; and, " growing bolder, I fet my foot to the ground without any pain at all. " After this I purfued the method prefcribed by the book, and the author's

* King Charles II.

" fon at Utrecht, and had a bruifed clove of garlic laid to the place that " was burnt, and covered with a large plaister of *diapalma*, to keep it " fixed there ; and when this was done, feeling no more pain, and tread- " ing ftill bolder and firmer upon it, I cut a flipper to let in my foot, fwel- " led as it was, and walked half a dozen turns about the room, without " any pain or trouble, and much to the furprize of thofe that were about " me, as well as to my own.

" For the pain of the burning itfelf, the firft time it is fharp, fo that a " man may be allowed to complain : I refolved I would not, but that I " would count to a certain number, as the beft meafure how long it lafted. " I told fix fcore and four, as faft as I could ; and when the fire of the " Moxa was out, all pain of burning was over. The fecond time was not " near fo fharp as the firft ; and the third was a great deal lefs than the " fecond. The wound was not raw as I expected, but looked only fcorched " and black ; and I had rather endure the whole trouble of the operation, " than half a quarter of an hour's pain in the degree I felt it the firft whole " night.

" After four-and-twenty hours I had it opened, and found a great blifter " drawn by the garlic, which I ufed no more, but had the blifter cut, " which run a good deal of water, but filled again by next night ; and " this continued for three days, with only a plaifter of diapalma upon it : " after which the blifter dried up, and left a fore about the fize of a two- " pence, which healed and went away in about a week's time longer ; but I " continued to walk every day, and without the leaft return of pain, the " fwelling growing ftill lefs, though it were near fix weeks before it were " wholly gone. I favoured it all this while more than I needed, upon the " common opinion, that walking too much might draw down the humour ; " which I have fince had reafon to conclude a great miftake, and that if I " had walked as much as I could from the day the pain firft left me, the " fwelling might have left me too in a much lefs time.

" I paffed that fummer at *Nimeguen*, without the leaft remembrance of " what had happened to me in the fpring, till about the end of September, " and then began to feel a pain, I knew not what to make of, in the fame " joint, but of my other foot : I had flattered myfelf with hopes that the " vapour had been exhaled, as my learned authors had taught me, and " that thereby the bufinefs had been ended : this made me neglect my

" *Moxa* for two days, the pain not being violent; till at laſt my foot began
" to ſwell, and I could ſet it no longer to the ground. Then I fell to my
" *Moxa* again, and burnt it four times before the pain went clear away, as
" it did upon the laſt; and I walked at eaſe as I had done the firſt time,
" and, within ſix days after, about a league, without the leaſt return of
" my pain.

" I continued well till this ſpring, when, about the end of March, feel-
" ing again the ſame pain, and in the ſame joint, but of the firſt foot;
" and finding it grow violent, I immediately burnt it, and felt no more
" after the third time; was never off my legs, nor kept my chamber a
" day. Upon both theſe laſt experiments I omitted the application of gar-
" lic, and contented myſelf with a plaiſter only of diapalma upon the
" place that was burnt, which cruſted and healed in a very few days, and
" without any trouble.

" I have ſince continued perfectly well to this preſent June, and with ſo
" much confidence of the cure, that I have been content to trouble myſelf
" ſome hours with telling the ſtory, which, it is poſſible, may at one time or
" other be thought worth making public, if I am further confirmed
" by more time and experiments of my own or of others. But this cure, I
" ſuppoſe, cannot pretend to deal with inveterate Gouts, grown habitual
" by long and frequent returns, by diſpoſitions of the ſtomach to convert
" even the beſt nouriſhment into thoſe humours, and the veſſels to receive
" them."

Thus far Sir William Temple: to which may be added, a remark made by Dr. Henry Bracken, an eminent phyſician in his time: he obſerves, that the averſion men have to endure pain prevents their experiencing many great cures which might be performed by fire; and adds, that he knew an inſtance of an obſtinate ſciatica, or hip gout, cured by a reſolute uſe of cauſticks: this application approaches the neareſt to actual fire, and perhaps the fire of Moxa may be of ſo mild a nature as to be next in degree to a cauſtick.

On the nature of Stones in the Bladder, and Gravel in the Kidneys, and the Author's examination of the question whether the ſame can by any means be broken or diſſolved.

A VALUABLE friend of mine, the late eminent Constantine Huygens, ſometime before his death, did frequently, both in converſation, and in his letters to me, expreſs a great deſire to know the reaſon why the Stone in the bladder produces ſuch great pain, and I having the ſame deſire of enquiring into the nature of thoſe Stones, procured ſome of them for my examination.

I put one of theſe Stones, which weighed about the fifty-eighth part of a pound weight, into a new glaſs, to which I applied ſo great a heat, that the ſalt and oil were in part expelled from the Stone, when the glaſs broke; I thereupon put it into another glaſs, and by the application of fire, I drew off the remainder of the ſalt and oil. Upon this I poured rain water, and I obſerved that they immediately united with the water. This mixture I ſuffered in part to evaporate, and then I diſcovered in it an incredible number of wonderfully minute ſaline figures, ſo ſmall indeed, that a million of them would not equal a grain of ſand; and with all my attention, I could not diſcover their exact ſhape, for the water or the particles in it impeded my view: however, I was perſuaded they had four ſides. I alſo examined ſome ſaline particles which adhered to the head of the glaſs, and I ſaw many of them to be thick and broad in the middle, and the ends terminate in a point. I alſo examined the veſſel in which I had put a little of the ſalt and oil, and here, beſides the before-mentioned figures, I ſaw ſome ſaline particles like *fig.* 21 and 22, and ſeveral like *fig.* 23, each of them with a riſing on the back, and all tranſparent like cryſtal.

I kept this water in my ſtudy for ſome days, frequently looking at it, and I always found that much of the liquid, as it ſeemed to my naked eye, did not evaporate; and at length I ſaw many ſaline particles in it like *fig.* 23; but theſe, except a few, were ſo very ſmall, that they could not be diſcovered without great attention.

Moreover, I poured rain water on the caput mortuum or aſhes, having firſt pounded it ſmall, in order to extract the fixed ſalt. I ſuffered this water in part to evaporate, and I diſcovered in it an incredible number of minute ſaline particles, which in many places were collected or coagulated together in great numbers, and yet retained their particular figures: this collection or coagulation of particles, bore ſome reſemblance to the figures of ſun flowers, ſo that in them I could not diſcover the ſaline particles, unleſs at the edges, where they repreſented the leaves of the flowers, as is ſhewn at *fig.* 24. In other places the ſaline particles lay in an irregular manner on one another.

I ſaw alſo many ſingle particles, of the ſhape ſhewn at *fig.* 25, all their extremities terminating in points. Upon keeping my eye long fixed upon a place where only ſome few ſmall particles of water lay, the heat which my touch occaſioned, (the air being then moiſt), cauſed a great number of particles to ariſe with one of their ends above the water; but, on the application of a little more heat, theſe newly formed particles diſappeared, and with ſtill more heat, ſome little vapour aroſe, and many of the ſaline particles were changed into a tranſparent watery liquor, but ſoon after all that liquor in many places was changed into irregular ſaline particles; theſe ſaline particles were ſo ſoft, that upon breathing on them they again changed into a fluid; but when I again applied a ſtrong heat, I ſaw them to be more ſolid. The particles of ſalt which had concreted in the ſhape of ſun-flowers, and ſome others of the ſame ſpecies, remained unaltered, ſo that I concluded both theſe were fixed ſalt.

Farther, I drew ſome blood from my finger, and mixed it with different parts of the water wherein was that fixed ſalt, and I obſerved, that the globules of blood from whence its redneſs proceeds, appeared as if their moiſt parts had been extracted by heat, for each globule appeared of a different ſhape, ſome flat, and I could ſee that they were compoſed of ſmaller globules; others looked like dead animalcules, whoſe feet diſplayed themſelves, and in a word, ſo many different figures, that I cannot rehearſe them all.

After this, I took ſome water wherein the volatile ſalt was diſſolved, and mixed it alſo with ſome blood, and I immediately perceived many of the globules of blood to be ſo altered or diſſolved, that they could not be diſtinguiſhed without great attention, ſome of them preſerving their figure longer than others; but this appearance I did not perceive, unleſs where a very little blood was mixed with much water.

Another of theſe Stones, in weight equal to about the twenty-ninth part of a pound, I applied to a very ſtrong fire, and obſerved the volatile ſalt, which proceeded from it, remained fluid ſo long as it continued warm; but when I took the black burnt part of it, it lay divided in many ſcaly parts like the coats of an onion, and that part which was not ſo divided, was the internal part of the Stone, which preſerved its round ſhape, and was not larger than the head of a large pin. This burnt ſubſtance, or caput mortuum, I weighed, and found it contained ten parts of a pound, ſo that nineteen parts, conſiſting of ſalt and oil, had been driven out of this Stone; and when I poured water on the reſidue, and ſuffered it to evaporate, I found, upon weighing, that it was reduced to eight parts. While the ſalt and oil I had extracted remained warm, I collected a ſmall portion of them, and ſaw that the oil, which was of a yellowiſh colour, by reaſon of the great number of ſaline particles in it, was not fluid.

After this, I put camphire in the water containing theſe ſalts, but I did not perceive them mix with the water, though the camphire ſtood in it for two days.

I afterwards poured off the water and poured ſpirits of wine on the ſalts, but I did not perceive them unite with the ſpirits of wine.

In all my obſervations, I never ſaw any of the ſaline particles in ſhape like the figures of our common ſalt, and I will venture to ſay, that the ſalt which we commonly uſe with our food, cannot produce any addition to the Stone in the Bladder, unleſs it could be proved, that ſuch ſalt can be converted into a ſtony ſubſtance in the body. But I know that many may not give credit to this, and alledge, that though the Stones in the Bladder may, in part, conſiſt of common ſalt, yet by applying fire to them in the way I have mentioned, they might aſſume the different figures I have deſcribed to be in the volatile and fixed ſalts extracted from thence. In order, therefore, to ſatisfy myſelf and others in this reſpect, I took ſome common ſalt and put it into a new glaſs, to which I applied ſo ſtrong a fire, that the glaſs became red hot, and the ſalt diſſolved into water: when the glaſs became cool, I broke it, and taking out the ſalt, I mixed it with fair rain water, and then obſerving it, I ſaw in the ſpace of half a minute, nothing floating in the water, except particles of common ſalt, but none like the fixed or volatile ſalt extracted from the Stone in the Bladder: when this burnt ſalt had ſtood for ſome hours, it was changed (the air being then moiſt) into a watery liquor, and upon applying ſome heat to it, nothing appeared except figures of common ſalt.

And ſince we ſee that Stones taken out of the Bladder do in part conſiſt of ſuch a fixed ſalt as is produced by infuſing the burnt matter in water, and which ſeems for a time to unite with the water, but ſoon afterwards coagulates, and becomes ſo hard, that though the water is boiled, this ſalt will not mix with the water; and alſo that by ſpirits of wine or camphire it cannot be diſſolved;

and that the volatile ſalt in it (for of theſe two ſalts the Stone in the Bladder does for the moſt part conſiſt) when expoſed to the moiſt air, will not diſſolve: that in the volatile ſalt there are particles ſo fixed that they cannot be diſſolved but by the force of fire; I cannot, for my part, perceive that any medicine can be found of ſufficient efficacy to diſſolve or break in pieces the Stone in the Bladder. And when we reflect on the ſharp-pointed particles of the volatile and fixed ſalts which, for the greateſt part, compoſe the Stone in the Bladder, we are not to wonder that thoſe Stones excite ſo much pain; eſpecially, if we conſider the ſharpneſs of ſo many wonderfully minute ſaline particles, and the many nerves in the Bladder, (for with any freſh accretion of matter new ſaline particles are added) which, with their ſharp points pierce and irritate the inſide of the Bladder.

I broke one of theſe Stones into ſmall pieces, and examined them by the microſcope, when I found that theſe Stones conſiſt of ſuch irregular parts as if we beheld a ſubſtance compounded of a great quantity of grains of ſand. And in theſe obſervations I often perceived thoſe wonderfully minute particles which I deemed to be ſalts, and which I have ſaid conſiſt of volatile ſalt, and which appear to be of the ſame nature with the calcareous ſubſtance in the limbs of gouty perſons. I have cauſed a drawing to be made of a very ſmall particle, broken off from the interior part of one of theſe Stones, partly to ſhew the formation of the Stone, and partly to exhibit the wonderfully minute and ſharp-pointed ſaline particles, but which I ſeldom ſaw in ſo great a number as are here ſhewn; the reaſon of which I take to be, that the particles of which the Stone conſiſts adhere together ſo very cloſely.

Fig. 26, A B C D E F G H, is this ſmall particle of Stone, which to the naked eye appeared the ſize of *fig.* 27. B C D E F, repreſent the very minute ſaline particles which are only to be diſtinguiſhed at the edges of this fragment. The Stone from which this piece was taken, together with ſeven or eight others, had been

fifteen years ago taken out of the Bladder of a child, after its death; the outſide of it was of an aſh-colour, very ſmooth, but under the outſide ſhell the whole Stone was as white as chalk, and, in ſome places, the ſaline particles of which it conſiſted glittered like fragments of glaſs.

I at different times mixed the water in which the volatile and fixed ſalts taken from theſe Stones had been infuſed, with a ſmall portion of blood taken from my finger, and found the globules of blood to be as it were diſſolved or ſeparated into minute parts by the mixture.

An intimate acquaintance of mine, and a perſon of eminence in this country, with whom I had formerly travelled, came to me in a ſtate of great pain, and complained, that now he was growing in years he was greatly afflicted with the Stone in his Bladder, and aſked me my opinion, whether any means could be deviſed to diſſolve or break it in pieces; to which I replied, that I conceived it to be impoſſible; and I alſo told him, that he had contracted this diſeaſe by his own manner of living. For, in our journies together, he had boaſted that he accuſtomed himſelf to bear hunger, thirſt, and cold; and he alſo told me, that in hunting he only held a ſmall leaden bullet in his mouth, and by continually moving it about, he could go for a whole day without drink, when others could not diſpenſe with it; and to which I had replied, that while we hold a leaden bullet in the mouth, its weight and coldneſs cauſes us to keep it continually in motion; and by the conſtant motion of the jaws, there is a continual ſecretion of ſpittle in the mouth, which we ſwallow, and thereby keep the throat moiſt, ſo that we do not ſo much require drink as other perſons, but nevertheleſs our bodies are not the leſs injured by the want of it. And the moiſture which is thus ſwallowed, and paſſes through the Kidneys, is very thick, and in ſmall quantity, and more obnoxious to the coagulation of ſalts and other particles of which the Stone in

the bladder is compoſed, than if we were to drink in ſufficient quantity.

Another gentleman of eminence, who was troubled with the Gravel, and had often been at my houſe, came to viſit me, with an old phyſician of ſome note, and brought with him a ſmall box, filled with Gravel Stones, aſking me, whether I thought there was any remedy which would diſſolve ſuch Gravel in the Kidneys; but this I pronounced was not to be found, by reaſon that theſe Gravel Stones do in great part conſiſt of ſuch hard ſaline particles, that they cannot be diſſolved otherwiſe than by actual fire, or very ſtrong aqua fortis; therefore, my opinion is, that we are only to look for a remedy to prevent the further increaſe or collection of them; and, I think, that to drink tea or coffee promiſes much for this purpoſe, becauſe thereby much liquid paſſes to the Kidneys; and further, that we ſhould drink plentifully of beer, between dinner and ſupper time, and not follow the example of thoſe who boaſt that they drink very little in the day, and none at all at ſupper.

In order to prove the truth of this my opinion, I put one of theſe Gravel Stones into a glaſs, and poured on it very ſtrong wine vinegar: this I ſet by for a whole year, in which time almoſt all the vinegar was evaporated, and, at the end of that time, I ſaw the Stone remain entire. I then poured more vinegar on it, and ſet it by another whole year, when the vinegar was almoſt all again evaporated; the Stone was then ſtill entire; but, upon ſqueezing it a little, it broke into many pieces, ſome of which were more than a thouſand times ſmaller than ſand; and I often ſaw a ſmall piece of this Stone, no larger than a grain of ſand, to conſiſt of many ſmaller particles concreted together; and the multitude of minute particles of which theſe Stones are compoſed appeared to me, in this inſtance, much more diſtinctly than I had ever ſeen them, or could have imagined; and the minute parts into which the Stone was broken were yet very hard.

Hence we may more certainly pronounce than before, that there is no medicine of ſufficient efficacy to diſſolve the Stones or Gravel in the Kidneys.

I have often heard in converſation, that medical men, and particularly ſurgeons, are accuſtomed to forbid thoſe who are afflicted with the Gravel, the uſe of ſome particular kinds of diet. But, for my part, I am well aſſured, that there is no kind of food in common uſe, which will not generate that calcareous matter which in gouty perſons is called chalk ſtones, and likewiſe Stones and Gravel in the Bladder or Kidneys. But, in ſhort, ſome phyſicians and ſurgeons talk of the efficacy of medicines in our bodies, and alſo of the conſtitution of our bodies themſelves, in much the ſame manner as blind men talk of colours.

On the nature and component parts of Gunpowder, and the quantity of air produced by its explofion: together with the Author's fentiments refpecting the fize of great Guns, and his obfervations on condenfed and rarified Air.

IT has long been my opinion, that all the faline particles which by the force of fire are elevated or driven upwards, from different fubftances, muft be of a globular figure; becaufe by the fire they are rendered foft, and are driven about with a very quick motion; in which opinion I was confirmed by recollecting the obfervations I had formerly made on Gunpowder. And now, to repeat thofe experiments, I provided feveral glafs veffels, which I made as clean as if they had newly come out of the glafs-houfe; fome of thefe as deep as the breadth of three fingers, others of four, and others of fix. Thefe I heated, in order to expel from them all the moift air of the atmofphere which we experience in this country in the autumn, and to introduce in its place a more rarified or thinner air than that in which we breathe. In a glafs fo prepared, I placed one or more of the largeft grains of Gunpowder, ftopping the glafs fo clofely as to exclude all the moift atmofpheric air; and I then applied to that part of the glafs where the Gunpowder lay, a fufficient degree of heat to fire the Powder. Hereupon the glafs appeared filled with a kind of white fmoke, in which could be feen, both at the bottom and fides of the glafs, the * charcoal and fulphur, which had entered into the compofition of the Gunpowder. When, in this

* It is generally known that Gunpowder is compofed of faltpetre, fulphur, and charcoal, in the proportions of three-fourths faltpetre, one eighth fulphur, and one eighth charcoal; the whole well pounded and mixed together, and afterwards prepared for ufe in the form of fmall grains, refembling feeds.

experiment, two, three, or more grains of Gunpowder were put into the glafs, the charcoal and fulphur were difperfed more forcibly than when a fingle grain was ufed; and I could plainly diftinguifh the particles of the fulphur from thofe of the faltpetre, for in fome places they lay fo thick together as to exhibit a yellow colour. The white fmoke (as it appeared to the eye) was in fact the faltpetre. This I immediately brought before the view of a good microfcope, and faw with great pleafure the inconceivable number of its component particles, of an incredible minutenefs, in a kind of circular motion, one among another; and, to my moft accurate infpection, they all appeared of a perfect round figure, befides feveral others which adhered to the glafs: after a fhort time, obferving that thefe moving particles began gradually to fubfide, I placed the glafs not in an upright pofition, but on its fide, that they might not fink to the bottom among the particles of charcoal and fulphur. When the glafs had remained in this pofition fo long that I judged all the faltpetre particles were in a ftate of reft, I again examined them, and faw with great furprize, that almoft all thefe particles which a little time before appeared globular, or to which I could not affign any other figure than that of globules, were now converted into hexagons, or falts of fix fides, fome of which were very exactly fhaped, others more irregular hexagons; and many of them, I could plainly difcern, rifing in a kind of piramidical fhape, like a pointed diamond. When I examined thofe parts of the glafs where the particles of faltpetre lay in confiderable numbers together, I could difcover but few of them to be of an hexagonal fhape, by reafon that the points of the particles (at the time they changed from globular to hexagonal figures) touching each other, they ftuck fo clofely together as to exhibit, upon the whole, only irregular figures. In another glafs I faw (among the figures before defcribed) fome of the particles of the faltpetre very long and thin, fome of which had the refemblance of

a bundle of arrows tied together, with their ends pointing different ways.

Beſides the foregoing obſervations, I ſaw, at the firſt firing or exploſion of the Gunpowder, a very thin moiſture in the upper part of the glaſs, which ſeemed to conſiſt of globules of different ſizes; which moiſt ſubſtance, I was well perſuaded, did for the moſt part proceed from the ſaltpetre, and which I ſhall therefore call oil of ſaltpetre, though it might perhaps be mixed with ſome of the oil of ſulphur. In order more fully to ſatisfy myſelf in this reſpect, I put into one of the glaſſes ſome refined ſaltpetre, leaving a ſmall opening at the top of the glaſs, that it might not burſt in the experiment, and then applied ſo ſtrong a heat to it that the ſaltpetre boiled very much; my only view in this was, to examine the vapour or moiſture which aroſe from the ſaltpetre, and to which I have given the name of oil; and I perceived, in the upper part of the glaſs, a large quantity of tranſparent and very fluid matter, which, upon inſpection, could be denominated no other than oil. At another time, I ſaw this ſubſtance or oil ſo collected or run together, that it appeared like drops of water ſpread irregularly upon the glaſs.

Not content with the preceding obſervations, I provided ſome other veſſels of very thin glaſs, in order to purſue my obſervations with more diſtinctneſs, and to ſee if I could not get a view of the changing of the ſaltpetre particles from globular to hexagonal figures, to which intent, immediately as the Gunpowder in the glaſs was fired, I brought the glaſs before the view of the microſcope, and then I ſaw the globular particles of ſaltpetre change their figure to hexagonal, and this was performed inſtantaneously, or as quick as a flaſh of lightning to the eye, not in one or a few of the particles, but all of them, as I may ſay, in a moment; and though at the bottom of the glaſs, where the greateſt number of particles ſubſided after their firſt motion ceaſed, I ſaw an immenſe number of ſmall particles lying to which I could not aſſign any

figure; yet I doubted not they were all of the ſame ſhape with the former: and I was confirmed in this my opinion, by obſerving now and then a larger particle among them, which was of the ſame ſhape I have mentioned. The number of theſe ſaltpetre particles produced from one grain of Gunpowder was ſo inconceivably great that I dare not make a computation of them, they were more in number than the particles of ſulphur and charcoal; and when I figured in my imagination any given particle, the ſize of a grain of ſand, to be divided into a thouſand millions of parts, I concluded in my mind, that the particles of ſaltpetre, ſulphur, and charcoal contained within a grain of Gunpowder, were much more numerous. Theſe obſervations were made with the greateſt diſtinctneſs when I uſed only one grain of Gunpowder; for when three grains were fired together, the ſaltpetre blew up with it the ſulphur and charcoal, the particles of which not only rendered the view indiſtinct, but alſo impeded the change of the globular figures into hexagons.

I have alſo taken notice, that if a grain of Gunpowder is fired at the bottom as it lies, then the ſulphur and charcoal it contains are blown up on high; but if it is fired at the top, then but little of the charcoal, and ſtill leſs of the ſulphur, are driven upwards.

Theſe obſervations cauſed me to reflect on the manner in which Gunpowder is proved amongſt us, to judge whether it be of a good quality: this is peformed by taking the quantity of a muſket charge of Gunpowder, and placing it in as cloſe an heap, of a conical ſhape, as may be, on clean white paper, and then giving fire to it with a burning match; and if, in this operation, the paper is neither burnt nor blackened, the Gunpowder is eſteemed to be good. Neverthelеſs, I cannot but think that much of this depends upon the perſon who gives the fire; for if the point of the burning match is applied to the upper ſurface of the Gunpowder, the conſequence muſt be, that the paper will be very much blackened, and alſo burnt, by reaſon that the upper part of the Gun-

powder firſt takes fire, and exerts its force on the lower part, which is laſt kindled, whereby the fire and ſmoke of this laſt part is driven downwards: but if the perſon who gives the fire does, with a light touch, ſo apply the match that it may fire the loweſt part of the heap as ſoon or ſooner than the upper part, it muſt follow that the lower part of the powder will blow upwards what lies on the upper part and ſides of the heap, and this before it is fully kindled, in conſequence of which the paper will be little injured. In a word, one man may ſo fire a parcel of Gunpowder that it will be deemed good powder, and another man may fire ſome of the ſame ſample in ſuch a manner that it ſhall be condemned as unſerviceable; that is, not ſufficiently ſtrong, or as being damp; for conſidering that ſome portion of time is required (as muſt certainly be the caſe) for the fire of one grain of Gunpowder kindled by another from beneath, to blow up the upper part of the ſame grain, much more time muſt be required in a whole muſket charge of Gunpowder for the fire of one grain to be communicated to the others.

I have alſo turned my thoughts on obſerving the firing of Gunpowder in a cloſe glaſs to the following conſideration, namely, whether the motion of the ſaltpetre particles and the fire did not leave in the glaſs a greater quantity of air compreſſed together. To make trial of this, I took ſeveral ſmall glaſſes, into one of which I put a large grain of Gunpowder, into another two, and into another three of ſuch grains, and, having perfectly cloſed all the glaſſes, I ſet fire to the Gunpowder in each of them, and left the glaſſes at reſt, until I judged that all the ſaltpetre particles were ſubſided, then opening the glaſſes, the air ruſhed out with violence.

I afterwards repeated this experiment with ſeveral other glaſſes, leaving ſome of them four, others five days untouched, and, upon opening them, the air ruſhed out with as much violence as in the former experiment.

Moreover, I took feveral other glaffes, and inclofed in each of them three grains of Gunpowder, which (the glaffes being firft carefully clofed) I fet on fire; but in this experiment fome of the glaffes flew in pieces, though they were all of the fame fize, and the quantity of Gunpowder the fame in each. The caufe of this I took to be, that in the broken glaffes the grains of Gunpowder might be fired altogether, or blown up more haftily than in the others. The glaffes which remained uninjured, after the fpace of twenty-four hours had elapfed, I opened in fuch a manner that the air within them (which in thefe glaffes was more compreffed than the common air we breathe, and more than the glaffes themfelves originally contained) might empty itfelf into a glafs globe filled with water, having a fmall neck, and placed not upright, but floping, in order that I might obferve accurately the quantity of air which was forced out of the glafs wherein the Gunpowder had been fired into the glafs globe filled with water; for fo much air as was forced into the globe, fo much water muft be driven out of it.

To afcertain this, I took feveral glaffes, and after having fired the Gunpowder in them, as before related, I weighed them, though to avoid prolixity, I will here only relate the computations I made with one of them. This glafs weighed feventy-feven grains, and when the compreffed air it contained had been made to pafs into the before mentioned glafs globe, I filled the other glafs with water, and weighing it, I found that the water contained in the cavity of the glafs weighed fixty-three grains: with this water I filled up the cavity in the glafs globe, which had been made in it, by introducing the air generated by the explofion of the Gunpowder, and I found that fifty-five grains of water had been driven out of the glafs globe, for I had only eight grains of water remaining after filling up the cavity or fpace; confequently, the firing three grains of gunpowder in the glafs, produced fuch a compreffion of air within it, that when the glafs was opened, nearly feven-eighth parts of the air it then contained rufhed out; fo that the air in the glafs, which before

firing the Gunpowder in it was of the fame denfity with common air, did, upon the firing, become fo condenfed or compreffed as to require, for attaining the natural ftate or liberty, an extenfion of fpace in the fame proportion.

With thefe obfervations I was not yet contented, and, among other experiments, I took a glafs, into which I put a common fized grain of Gunpowder, leaving at the extremity of the glafs an opening the fize of a common pin; this opening, which was at a thin and pointed end of the glafs, I immerfed in the before mentioned glafs globe filled with water, and upon firing this fingle grain of Gunpowder in the glafs, fo great a quantity of air was driven into the glafs globe, that to replace the water which was thereby expelled, I had need of one hundred and fixty grains of water, over and above the water which had found its way out of the globe into the glafs veffel wherein the Gunpowder was fired. This laft mentioned glafs veffel (which with one more were the only two of feveral ufed by me, had remained entire; for many of the glaffes upon the rufhing in of the water, burft in pieces), I weighed before I fired the grain of Gunpowder, in order, after the firing, to know the fize of the cavity it contained, and found that the water in it weighed one hundred and fifty grains.

And now, to make a calculation with as much accuracy as poffible, we muft lay out of the cafe, the fize of the cavity of the glafs wherein the Gunpowder was fired, and fay only this, a common fized grain of Gunpowder, in its explofion, forces fo much air into a glafs globe, as fills a fpace equal to that occupied by one hundred and fixty grains in weight of water; and to make a comparifon (though it will be a very uncertain one) between the fize of a grain of Gunpowder and the quantity of air it produces on being fired, we will fay, that water and Gunpowder are of equal weight, though this is not fo in fact, for Gunpowder finks in water, and therefore is heavier. Now let thirteen grains of Gunpowder be confidered to make the weight of a grain, thefe thirteen grains of Gunpowder

multiplied by 160, the product is 2080, and ſo many times more ſpace does a grain of Gunpowder occupy when fixed, than what it did before.

This is then the magnitude or quantity in ſpace of the expanſion or exploſion of Gunpowder, computed by me with all the accuracy I was able, though by no means to be deemed perfect, becauſe one grain of Gunpowder will explode much more completely than another, by reaſon that the mixture of ſaltpetre, ſulphur, and charcoal, is not in their proportions alike in all, as a proof of which, I have obſerved in firing a grain of Gunpowder, that part of it has been conſumed before the reſt blew up.

But one circumſtance in theſe my obſervations appeared moſt worthy of note, namely, that with the ſame velocity as the air ruſhed out of the glaſs wherein the grain of Gunpowder was fired into the glaſs globe, with equal velocity was the water forced out of the glaſs globe, through the narrow opening of the glaſs where the Gunpowder had been fired, inſomuch as almoſt half to fill that glaſs with water; the cauſe of which, I conceived, could be no other than this; the fire produced by the kindling of the Gunpowder incloſed within the glaſs, requires great extenſion, whereby the air in the glaſs muſt in part be driven out, and whereas with the ſame rapidity as the fire was kindled, it will eſcape through the pores of the glaſs into the air, ſo upon the fire's quitting the cavity of the glaſs, ſome other ſubſtance muſt follow in its place to occupy the vacuum, whereupon this vacancy in the glaſs muſt be filled with water, becauſe the opening in the glaſs is immerged under the water.

From this regurgitation or ruſhing of the water into the glaſs, when the Gunpowder was fired, after the fire had eſcaped from thence, I was led to reflect on the firing of Gunpowder in a Cannon or great Gun, and I concluded that a ball fired from an overlong great Gun would not have ſo much force, nor be driven to ſo great a diſtance as if the Gun was of a more moderate length, for

I argued thus with myfelf; in a Gun eighteen feet long, half as much time again is required for the Gunpowder to drive the ball to the muzzle of the piece, than in a Gun which is twelve feet long; in which length of time, much of the Gunpowder in the long Gun is confumed, before the ball gets into the open air; this confumed Gunpowder is not only wafted, but no fooner is the Gunpowder exploded, than immediately nearly one-fourth part of the expanfion efcapes out of the Gun, which fpace muft be filled up by fome other fubftance, and this is fupplied from that part of the Gunpowder which firft takes fire, and thus the force of the powder is diminifhed before the ball has quitted the Gun, in like manner as I have before faid, refpecting the firing of Gunpowder in a glafs, namely, that fo foon as it is fired and the flame is fpent, the air within the glafs is rendered fo thin and fubtile, that about a fourth part of the expanfion produced by the firing of the Gunpowder muft be replaced in order to fill the glafs again.

In converfation with a certain gentleman of high authority in military matters, who came to vifit me in company with fome foreigners of rank, I mentioned my opinion of the inutility of too long great Guns, (for our difcourfe turned upon that fubject); and he informed me, from his own experience, that upon firing two guns, one of eighteen feet long, and the other of fourteen feet long, in order to prove their greateft power, he had feen the gun of fourteen feet long, carry its ball much farther than the other, which was eighteen feet long, and hereby I was confirmed in my before mentioned opinion.

It was formerly my firm belief, that there could be no other reafon of the great explofion produced by the firing of Gunpowder, than firft, from the faltpetre particles being put into a violent agitation and divided into fmaller parts; and fecondly, from the fire, and that when thofe particles fubfided to a ftate of reft and the fire efcaped, the remaining fubftances left no greater fpace than they had occupied before the faltpetre was by the fire divided into many

parts ; but now we fee the contrary to be the fact, for the particles of air in a clofed glafs, wherein Gunpowder is fired, are fo compreffed together, that feven parts out of eight will efcape out of the glafs upon its being opened.

I have endeavoured to fatisfy myfelf upon this fubject, by faying that the particles of air in their natural ftate in the glafs, before the firing of the Gunpowder, were, upon fuch firing, divided into ftill fmaller particles, and that thofe fmaller particles being of a globular figure, muft (fo long as they remain diftinct and not forced one into another) touch each other on their fuperficies, and leave between them in the remaining fpaces, fome more thin and fubtile fubftance; whence it muft follow, that the particles of air within the glafs, will require a greater fpace than they occupied before; but on the other hand, I confidered, that though every particle of air which was in the glafs before the firing of the Gunpowder might, upon the firing it, be divided into a thoufand fmaller parts, this would not produce fo great an expanfion as we experience ; for, let a fack of wheat be ground into fine flour, every grain will be divided into a thoufand parts at leaft, and though the wheat is divided into many fmall particles, it will not occupy feven or eight times greater fpace than it did before the grinding. In fhort, with all my obfervations refpecting the great expanfion or explofion of Gunpowder when it is fired, I have not been able to explain the matter to my own fatisfaction.*

* Mr. Leeuwenhoek, in this Effay, attributes the force of Gunpowder, when exploded, to fome alteration produced by it in the circumambient air, rather than to the production or generation of air from the Gunpowder itfelf ; and though in fome few places he expreffes himfelf in a manner, intimating, that air is generated by Gunpowder when fired, he does not feem to have been fully informed of the doctrine now eftablifhed, namely, that Gunpowder, or rather its principal component, Saltpetre, contains in it a great quantity of air condenfed as much as poffible, which, when fet at liberty, upon the firing the Gunpowder, does, by its elafticity or expanfive power, produce the violent and furprifing effects we obferve. Mr. Leeuwenhoek's experiments do, however, confirm that doctrine, and they are themfelves illuftrated by it.

But when, from the preceding obſervations, we reflect on the time that Gunpowder, when fired, requires to drive the ball out of a Gun, and how much of the charge of powder is conſumed in that time; which conſumed Gunpowder is injurious to the effect of the firing, and alſo when we conſider how much of the charge may be driven out of the Gun before it takes fire, the concluſion is, that it is worthy of conſideration to deviſe ways and means whereby the whole charge of Powder in a Gun may, if poſſible, be ſet on fire all at once; and if this be accompliſhed, ſo much powder will not be required for a charge as is now in uſe.

I will now explain the manner in which I make the foregoing experiments on the exploſion of Gunpowder: I take a glaſs tube of the ſhape and ſize repreſented in Plate XV. *fig.* 26, L M, at one end of which I blow the glaſs globe H I K M, then, after dropping into it a grain of Gunpowder, I give the glaſs the ſhape repreſented at *fig.* 27, P O N Q R, whereby the grain of powder fixes itſelf in the tube at the place marked N. I then take a ſmall pipe, called by ſilverſmiths a blow-pipe, which they uſe to ſolder their work, and, firſt putting on my ſpectacles to defend my ſight, I bring the tube near my eye to ſee the effect of the firing; then directing the flame of a candle or lamp, by means of the blowpipe, to the place where the grain of powder is lodged, it immediately takes fire, producing the inconceivable number of particles in violent agitation, reſembling ſmoke, as I have before deſcribed; beſides the multitudes of particles adhering to the glaſs, and indeed, the number of particles into which a grain of Gunpowder is divided, cannot be conceived, but by making the experiment.

I then blow a glaſs of the ſhape repreſented at *fig.* 28, S T V W, taking care that it be ſomewhat thicker at the part T, and filling it with water, I hold it in the oblique poſition repreſented in the figure.

After this, I infert the point of the tube reprefented at *fig.* 27, wherein the Gunpowder was fired into the aperture of the glafs, *fig.* 28, at W, fo that the bent part of the tube R, may prefs the bottom of the glafs at T, and continue the preffure until the tube breaks about Q, whereupon the compreffed air in the cavity of the glafs, *fig.* 27, rufhes violently into the glafs, *fig.* 28; but as it cannot efcape through the aperture W, it places itfelf about V, by which means the fame fpace of water is expelled at W, as of air is forced from the glafs, *fig.* 27, into the glafs, *fig.* 28, and upon weighing this laft mentioned glafs, when filled with water, and again when part of the water, as before mentioned, has been driven out of it, I can make an exact computation of the quantity of compreffed air driven out of the glafs, *fig.* 27.

Again, I take another glafs of the fhape reprefented at *fig.* 27, w iththis difference only, that the tube at Q is ftraight, and the aperture left unclofed, and then infert this into the glafs, *fig.* 28, fo as not quite to touch the bottom; then, upon firing the grain of Gunpowder, the condenfed air rufhes with great force into the glafs 28; after which firing, and the heat of the flame efcaping out of the glafs 27, inftantly, as I may fay, the water makes its way through the orifice Q, into the glafs 27, in greater quantity than one could imagine.

In thefe experiments, by the firing of the Gunpowder and the rufhing in of the water, the glaffes are often broken; therefore, in making thefe obfervations, it will be neceffary to repeat the experiments many times.

Of the LOUSE.

THIS animal, which is fo troublefome to many, efpecially the poor, who have not the means of frequently changing their linen and other apparel, is by fome writers fuppofed to be produced from dirt, fweat, or excrements: but to convince fuch perfons of their miftake herein, I will give the defcription of feveral parts of this creature, as examined by me, the perfect and wonderful formation of which, will clearly prove that thefe animals cannot be produced otherwife than by the ordinary courfe of generation.

Plate XVI. *fig.* 1, A B C D E F G, is the head of the Loufe, in which may not only be feen two very black eyes, but alfo two perfectly made horns with joints, befprinkled with hairs in many places, as fhewn at C D and G F. The letters H I K B indicate only the outline of a part of the Loufe's body.

When I was preparing this object for the limner, I cut off the head and breaft from the lower part of the body, and placed this fmall part only before the microfcope; for when I placed the animal before it entire, it was in fuch continual motion, bending its body backwards and forwards, that it was impoffible to obtain a diftinct view of it, fo as to make a drawing; and this piece of the body remaining fixed before the microfcope, the horns and feet continued in motion for an hour.

In the contemplation of feveral lice when placed before the microfcope (and I had plenty of them brought to me for my money), I received great pleafure in contemplating the motion of the internal parts in the head and feet, and even in the œfophagus or gullet, which in this creature lies partly in the head, and through which the blood it fucks may be feen running very fwiftly: this

motion in the gullet was feen in a clear liquor paffing upwards and downwards alternately.

At E is a protuberance fomewhat like a nipple, which, when the Loufe is preparing to take its food, it projects further, and from the extremity of it thrufts out its * piercer, in order to fuck the blood. This piercer, or rather the piercer with its fheath (for there are two of them, one inclofed in the other), I have often taken out of the Loufe's head, but it was not till after repeated trials, by reafon of their exceffive flendernefs, that I could place them before the microfcope, fo as to give a drawing of them both.

Fig. 2, L O P, is part of the Loufe's head; at O is fhewn the protuberance, which I have likened to a nipple, as it appears when the piercers are protruded out of it: O M is the larger piercer, or, more properly fpeaking, the fheath which contains the piercer M N, both of which are drawn into the head when not in ufe. At N, the extremity of this piercer appears a little fplit or divided. *Fig.* 3, K L, is this fheath taken out of the head, and L M is the exceffively flender piercer it contains, protruding beyond the extremity of the fheath.

When the Loufe is about to feek its food from the human body, it extends the nipple or fnout I have defcribed, from its head, and from it protrudes the fheath of the piercer; and laftly, the piercer itfelf, which being introduced between thofe fmall fcales with which the furface of the fkin is covered, it pierces the blood-veffels lying underneath and then fucks the blood, which is its nourifhment; and in doing this, it places itfelf upon its head, the more readily to introduce the piercer into the body. The blood which the animal fucks, may be feen paffing with a very fwift motion into its body, and it is a curious fpectacle to behold the rapid motion with

* The Dutch word is *Angel*, which the Latin verfion renders *Aculeus*, both words fignifying a fting: but the Tranflator has adopted the word piercer, as more applicable to the ufe which this part of the animal is defigned for: befides, we fhall fee prefently, that the fting of the Loufe, properly fo called, is placed in a different part of its body.

which the human blood, when received into the Loufe, is inceffantly driven to and fro both in the ftomach and inteftines, to prevent its coagulating, which would be fatal to the animal.

In my experiments and obfervations on this creature, although I had, at feveral times, had a number of them on my hand drawing the blood, yet I very rarely felt any pain from their punctures, which is not to be wondered at when we confider the exceffive flendernefs of the piercer I have been defcribing; for, upon comparing this with a hair taken from the back of my hand, I judged, from the moft accurate computation I could form by the microfcope, that the hair was feven hundred times the fize of this incredibly flender piercer, which, confequently, by its punctures, muft excite little or no pain, unlefs it happens to touch a nerve. Hence I have been induced to think, that the pain or uneafinefs thofe perfons fuffer who are infefted by thefe creatures, is not fo much produced from the piercer, as from a real fting which the male Loufe carries in the hind part of its body.

Fig. 4, B C D, reprefents part of this fting, fo far as it is protruded out of the animal's body, and thus far it is of a hard fubftance and a yellowifh colour like the claws: in it may be feen a kind of groove or cavity, which paffes through the whole fting, and feems as if it were defigned to convey fome liquor to the extremity when the animal makes ufe of it, though I never did actually obferve any fuch liquor. In this figure the fting is drawn with the cavity fronting the eye, in which pofition it appears quite ftraight. *Fig.* 5, E F G, is the fame part of the fting viewed fideways, in which pofition it appears to be of a curved fhape, and thus far it was bare of any mufcular parts: the lower part G H I E, in its natural ftate, is covered with mufcular and flefhy parts, which, being cleared away, there only remains to be feen in the figure, the hard or horny part of the fting, which gives it ftrength and firmnefs.

This fting is the Loufe's weapon of offence, and which it ufes when preffed by the clothes or otherwife difturbed; for I have ob-

ferved that, when roughly handled, they protrude their fting as preparing to ftrike ; but as to the reafon why the males alone are provided with it, and not the females, I have formed fome conjectures, but not fo as to fatisfy myfelf in that refpect.

The feet and claws of this animal difplay the perfect contrivance manifeft in the formation of fo fmall a creature. *Fig.* 6. A B C D E F, is one of the fix feet of the Loufe ; B C D, is the largeft claw, one of which the Loufe has on every foot, and, when the animal is on that part of the body where there are no hairs, it lays hold of the fkin with this claw, in order the more forcibly to introduce the piercer which it carries in its head into the body, in order to fuck the blood ; but when moving from place to place, or not employed in fucking the blood, it does not cling to the body, but to the fhirt or other garment (on which alfo it lays its eggs), becaufe it can eafily fix its claws into the filaments of the linen or woollen.

To lay hold of a hair it grafps it with this claw and the prominent part of the foot, which is fhewn at D, and which is alfo provided with a very fmall nail or claw, and more particularly the part E, which I call the Loufe's thumb, and which is alfo furnifhed with a fmall nail. For in like manner as we, in holding any thing in our hands, ufe the thumb, fo does the Loufe in grafping a hair, make ufe of what I call its thumb. A F G, is that part of the Loufe's leg which joins the body.

I could have given a drawing of the Loufe's claw magnified to a greater fize than the whole foot is here reprefented, but I do not make ufe of fuch very deep magnifiers, unlefs neceffity requires it ; for I think that an object is fufficiently magnified when we can fee all the parts of it diftinctly.

Upon exhibiting this creature before the microfcope to a certain great perfonage, he obferved to me, that his foldiers, who were infefted with lice, found them more troublefome in rainy than in dry weather : for which I gave this reafon, that the clothes, when wetted, fhrink and comprefs the body fo clofely, as to impede the

Loufe in its motions, and caufe it to ufe the fting which it carries in its tail, whereas, in dry weather, the clothes hanging loofe on the body, the Loufe has room to infert its piercer and fuck its food, which it cannot do without bending its body and raifing its hind parts.

The Loufe is fo prolific an animal, that it is a common vulgar faying, that it will be a grandfather in the fpace of twenty-four hours. This I could never believe to be the fact, but rather that it would require nearly a month for the offspring of a Loufe to be capable of producing young of its kind; and, in order to make proof of it by experiment, I at firft propofed to hire fome poor child to wear a clean ftocking for a week, with two or three female lice in it, and well tied or fecured at the garter, in order to fee how many young ones would be produced in that fpace of time; but I afterwards confidered, that I could make the experiment with much more certainty in my own perfon, at the expence only of enduring, in one leg, what moft poor people are obliged to fuffer in their whole bodies during all their lives.

Hereupon I put on one leg, inftead of the white under ftocking I ufually wear, a fine black ftocking, chufing that colour, becaufe I confidered that the eggs and the young lice thence proceeding, would be more eafily diftinguifhed upon it. Into this ftocking I put two large female lice, and cutting another black ftocking into long flips, I bound it over the firft above the knee, to prevent their efcaping. After wearing this ftocking fix days, I took it off, and found one of the Lice in the fame place where I had put it, and that it had laid fifty eggs, and in another part of the ftocking the other had laid about forty eggs, but the parent I could not find. I opened the other which had laid the fifty eggs, and found in its body at leaft fifty more, and who knows how many eggs it had laid before I put it into the ftocking, and how many more eggs it might then have in its body which my fight could not reach?

Having worn the ftocking ten days longer, I found in it at leaft

twenty-five lice of three different fizes, fome of which I judged were two days old, others a day old, and the reft newly come out of the egg, befides others ready to come forth, as I found upon opening one of the remaining eggs. But I was fo difgufted at the fight of fo many lice, that I threw the ftocking containing them into the ftreet; after which I rubbed my leg and foot very hard, in order to kill any Loufe that might be on it, and repeating the rubbing four hours afterwards, I put on a clean white under ftocking.

I have caufed a drawing to be made of the Loufe's eggs, (commonly called nits), as viewed by the microfcope, to fhew from what part the young Loufe iffues forth. *Fig.* 7, N O P, is the egg, Q R, is a hair of the wool to which the egg is fixed, by means of fome gummy fubftance, which the Loufe emits from its body together with the egg. At N, is fhewn how part of the egg is fhaped, fomewhat like a lid or cover, which in all probability is made very tender or brittle, that the young Loufe, when grown to its full fize within the egg, may be able to break it open and iffue forth; whereas other young animals, who are provided with teeth or pincers, can gnaw or bore holes in the fhell. *Fig.* 8, S T V, is another egg, wherein, at T, may be feen that the fhell of thefe very fmall eggs is provided with a fkin or membrane, the fame as a hen's or other bird's egg; and under the lid or cover of this egg, which the young Loufe breaks open, there is another thin membrane, which is alfo broken by the young Loufe, and is to be feen at T.

Now fince we fee, by experiment, that a Loufe in the fpace of fix days can lay fifty eggs, and have as many more remaining in its body, we may eafily conceive, that a poor perfon who has an hundred female Lice about his body, and has not any change of clothes or linen, and who, moreover, through floth is carelefs of deftroying thofe he has about him, may in a few months (if I may ufe the expreffion) be devoured by thefe vermin.

To give a clearer conception of the great increafe of thefe animals, let us fuppofe a perfon to have about his body two male Lice, and as many females, and that the females in twelve days time lay two hundred eggs, and that, fix days afterwards, out of thofe eggs are produced an hundred males and as many females, and that this young brood in eighteen days time are grown to a fize to propagate their kind, and that each of thofe young females in the fpace of twelve days more lays an hundred eggs, from which, in fix days time, other young lice are produced; upon this fuppofition, the number of lice fpringing from two females, will amount to ten thoufand. Thus it appears that two females may, in eight weeks time, be grandmothers, and fee ten thoufand lice of their own offfpring, which, unlefs reduced to actual demonftration, would feem incredible; and who can tell, whether in the heat of fummer thefe creatures may not breed in half the time I have mentioned?

I will here put an end to this loufy difcourfe, which has gone to * three times the length I intended; but my defign in it was, to fatisfy myfelf, and to convince others, that fome days are required for the egg laid by a Loufe to produce a young one, befides the time required for eggs to be again laid by fuch young one, and alfo to afcertain in what numbers thefe creatures do multiply.

* In the original, the author is much more prolix and circumftantial than here fet down; but the Tranflator has thought it fufficient, on fuch a fubject, to give the fubftance and general refult of his obfervations.

Of the MITE.

THE Mite is the fmalleft animal that I have ever obferved about our houfes, but in every kind of dried provifion, fuch as ham, bacon, and dried fifh, they are almoft always to be found.

In reflecting upon this animal, I was defirous to know the nature of their propagation, and how long time their eggs would take from their being firft laid to their being hatched, and alfo in what fpace of time a young Mite will come to its full growth.

In catching thefe creatures, I found they were endued with a very quick fight, for after I had once touched them with the inftrument with which I caught them, they afterwards avoided it in a manner which furprifed me.

I had often feen the eggs of Mites in cheefe and other fubftances, and I now proceeded, by the help of a magnifier, to open fome of the largeft mites, which I judged to be females, and viewing them by a microfcope of ftill deeper magnifying power, I faw at three feveral times, not only the eggs, but alfo through the fhells of them I perceived their infide to be compofed of greater and fmaller globules, exactly fimilar to thofe in the yolks of hen's and other birds' eggs, only with this difference, that the globules in the yolk of a hen's egg are each of them larger than the entire egg of a Mite.

I took a glafs tube, into which I put a piece of bifcuit and five or fix Mites, and then, by the help of fire, I clofed the orifice of the tube, fo that no egg laid by the Mites could drop out; the firft day of their confinement I found one egg, the next four, and the third fix eggs, and one of the Mites dead: the fourth day I counted as many as twenty eggs, and afterwards ftill more, but

the number I did not particularly reckon ; and as it was in the middle of October, and the weather growing cold, on the fourth day after the firft egg was laid, I put the tube into a leather cafe, and carried it in my pocket, to fee what time the eggs would require to produce young Mites. On the eighth day I faw a young Mite, and the following day three fmall Mites creeping about the glafs, and then I faw only one of the Mites I had put into the glafs remaining alive; and, upon minutely examining thofe that were dead, I judged that they had been almoft wholly devoured by the furvivors. The next day I faw five or fix fmall Mites, but I was furprifed to fee thofe young Mites had only fix feet, whereas thofe which were grown larger had eight.

By the microfcope I every day examined the eggs, and when they were about four days old, I could diftinguifh the fruitful from the barren ones; thofe which I deemed fruitful were of a dark colour throughout, whereas the barren ones were one half dark and half tranfparent, which tranfparency was occafioned by the globules which compofed the egg having funk to the bottom. Afterwards I faw that from all thofe eggs which had remained dark throughout, the young Mites had crept out, leaving nothing but the fhell behind them; whereas thofe eggs which were in part tranfparent, remained entire.

Not fatisfied with thefe obfervations, I took feveral glafs tubes, into each of which I put fome fmall pieces of dried fifh, and thefe tubes being clofed at one extremity, I placed the open ends near a bag of meal, in which I was certain there were many Mites, in order that the Mites might of themfelves, without being hurt, or being mixed with any dirt, remove into thefe glafs tubes to the pieces of fifh, which accordingly came to pafs; for, the next morning, I found in fome of the tubes fifty Mites, as well large as fmall ones, and fome fo minute, that they feemed to be newly hatched, and had only fix feet.

In order that thefe Mites might not want air, I clofed the ori-

fices of the tubes fo as to leave an opening at the end in fome of them, as far as my eye could judge, about the fiftieth part the fize of a Mite's egg, and others larger: thefe glaffes I then placed before the microfcope, for thefe creatures, by reafon of their minutenefs, are almoft invifible, and by my naked eye I could not perceive they had any life in them.

I have given a drawing of the Mite, as feen through the microfcope, to fhew its fhape as nearly as the limner could imitate it.

Plate XVI, *fig*. 9, A B, reprefents the Mite's egg, the fhell of which is covered with fo many fharp prominent parts, that the whole appears dotted: thefe eggs may be feen in great numbers, in the rinds of cheefe, efpecially when the cheefe is old and Mites are difcovered in it. *Fig*. 10, C D E F, is a Mite feen through the microfcope when fixed to the point of a needle. This creature has eight feet, in each of which the joints are reprefented as accurately as could be done; moreover each foot is furnifhed with two wonderfully minute crooked claws at its extremity, with which the animal can readily grafp any thing, and fupport its own body; and I have feen a Mite, when fixed to the point of a pin, lay hold with its claws on the hair of another Mite and fufpend it in the air; and I wondered that a fingle hair of a Mite fhould be fo ftrong as to hold the animal's body fufpended by it. At D is fhewn the head of the Mite, the fore part of which is fo very fharp, though with an opening on it, (from which opening I have feen fomething like a tongue put forth), that the mouth might be fitted to bite afunder the fibres of the flefh on which it feeds, as I have feen in the feet of a loufe gnawed by a mite, leaving oblong fcratches. On each fide of the head was a crooked hair, beginning at the thicker part of the head, and ending near the extremity, as if it were alfo fixed there, which at firft I thought it was, but having often feen that when a Mite touched thefe hairs, while cleaning its head, in the manner that cats and other animals do, the hairs when moved out of their place recovered themfelves with a

kind of ſpring, whereupon I thought that theſe hairs might be deſigned to protect the eyes.

The Mite here pictured was not one of thoſe taken out of the meal or cheeſe, but a piece of ſmoke-dried meat, which abounded with them; theſe Mites were of the ſame ſhape with others, but ſomewhat larger, which I preſumed might be owing to the abundance of moiſt nouriſhment they got from the meat, which thoſe in the meal had not; and when I put ſeveral of thoſe Mites from the dried meat into a glaſs, they could not move about readily, but remained almoſt fixed in the place, or moved very ſlowly, becauſe they could not fix their claws in the glaſs, and the hairs on their bodies, by the fatty particles they had contracted, ſtuck to the glaſs.

I have obſerved theſe animals remain many weeks without food, and alſo to endure the cold, and even lay eggs in cold weather, which in about a month's time produced young Mites.

And now, if we contemplate the wonderful regularity, as well in the formation, as in the propagation of this animal (the minuteſt as I have before obſerved, that is found about our houſes, and by reaſon of its minuteneſs, unknown to or unobſerved by many), muſt not the moſt judicious philoſophers agree with me in opinion, that as it is impoſſible for an elephant to be brought forth from duſt or dirt, it is equally impoſſible for a Mite to be bred out of meal or any corrupted ſubſtance, or in any other manner than the regular way of generation I have deſcribed.

Of a Maggot, which feeds on the graſs in meadows, and a Fly bred from it in the Spring: with ſome remarks of the author on the Graſshopper and the Locuſt.

IN the month of May I was ſhewn, by a countryman, a meadow which, though good land, was very thinly covered with graſs, and the reaſon he gave for it was, that a certain ſpecies of black, thick, and ſhort maggots devoured the roots of the graſs; and he added, that the graſs would not grow, until there ſhould be ſome hot weather, by which theſe maggots (called in our language *den Hemelt)* would be killed. I aſked him whether he ever obſerved that theſe Maggots changed into ſmall Graſshoppers, or into any other flying animal, but to this I received no other anſwer than, that the Maggots would ſoon diſappear if there were but a few hot days, and then the graſs would grow plentifully: and he farther ſaid, that the lower marſhy lands, in which graſshoppers are ſeldom ſeen, were moſt infested with theſe creatures. And the country people ſay, that after a few hot days they often ſee theſe maggots lying dead in the fields.

This information, which I received from different countrymen, was not ſatisfactory to me, for I judged that if theſe Maggots were accurately obſerved we ſhould ſee a different appearance, namely, that they would be found to change into flying animals, and the rather, becauſe theſe Maggots are not found in numbers together, but diſperſed about ſingly.

About the beginning of May I went to a meadow which was but thinly covered with graſs, and took with me a countryman, an intelligent perſon, who lived by keeping oxen and horſes at paſture:

this man ſearched for the Maggots with me; I put ſome of them into a box, but in a ſhort time they partly ſhrivelled up and died; others of them I put on a turf of graſs, which I placed in an earthen veſſel, and watered it every day; this veſſel I ſet in my ſtudy, that I might remember to view it frequently, and when the graſs began to wither, I replaced it with a freſh turf: in doing which I ſaw more of the Maggots lie ſtill on the earth, I thereupon took the turf which began to wither, and, taking from it the Maggots, I placed them on the freſh turf: and this I continued to do till the end of July, in all which time I perceived no alteration in the Maggots. In the mean time I chanced to be in the company of ſome gentlemen of reſpectability, with whom converſing on this ſubject, they told me that theſe Maggots would be changed into flying animals, called, in our language, *Speketers*. But, though I was well aſſured, in my own mind, that ſome ſuch change would take place in theſe creatures, I determined to ſupply them with freſh earth, until I ſhould ſee their transformation.

In the beginning of Auguſt the turf again began to dry, and, conſidering that ſo long as three months time had now elapſed, I examined the turf, and took the Maggots out of the earth, when I ſaw one of them, which was whiter than the others, to be ſomewhat contracted, whence I judged that its transformation was at hand; whereupon I placed it and two others of the Maggots in a box, and the next day I ſaw one of them was changed into an aurelia or cryſalis, having put off its ſkin, which lay beſide it. I ſought for another of the Maggots which was changed into a cryſalis, but which I had not ſhut up in a box, and, not finding it, I concluded it had changed into a flying animal and eſcaped. The other Maggots were in a ſhort time contracted, but did not undergo any further change.

Plate XVI. *fig.* 11, A B C, repreſents the Maggot called *den Hemelt*, in which, from the month of May to Auguſt, I perceived no alteration in ſhape. *Fig.* 12, D E F, repreſents the Maggot changed into an aurelia or cryſalis, which at firſt was in frequent motion,

and did not lie ftill till that part which was its back was placed uppermoft. *Fig.* 13, G H, is the fkin put off by the Maggot.

Two days after I had perceived the change I have been defcribing in the Maggots, I went to the meadow where I had found them, where I faw a great number of creatures flying about, which our children call *Speketers,* and others *Mayers ;** these are reprefented at *fig.* 14, I K L M. Some of thefe flying animals I brought home in a box, to fee whether they contained any eggs, but I could find none, and the animals feemed to me to be all of the fame fhape. I again fearched in the meadow, to fee whether I could find any difference between the males and females, but without fuccefs.

In the mean time I faw, flying in my garden, a fingle animal of the fame fhape with the before mentioned ones, with this difference only, that it was fomething larger and its tail terminated in a point, whereas thofe of the others were blunted at the end. This animal I opened, and took out of its body a great number of oblong black eggs, infomuch that, on counting thofe taken out of this one animal, they exceeded two hundred. Others of thefe animals I found in the town, all containing eggs, but I did not find one in the town of the fame fhape as thofe which I had taken in the field, which made me doubt whether both were of the fame fpecies.

In the beginning of September, on going again to the meadow, I did not at firft fee any animal, except thofe I firft defcribed, and which are reprefented at *fig* 14, but after I had ftood there a while I faw one of the fame fort with thofe from which I had taken the eggs. This creature, fettling on the ground at a fmall diftance from me, extended the hind part of its body downwards through the grafs, and remained in that pofture a fhort time: this it did three or four times while I was looking at it, forcing its way between the blades of grafs fo clofely, that when I wanted to take it, I could not

* From the figure given of thefe animals, it fhould feem that they are what are called, by children in England, father long-legs, and in Summer evenings are often feen in our houfes.

eaſily ſeparate it from them, and I ſaw this performed by three or four others of the animals while I ſtayed in the meadow.

From theſe obſervations I concluded that both the animals I have mentioned were of the ſame ſpecies, but thoſe which I had taken firſt were all males, and thoſe from which I extracted the eggs were females, the females being of a larger ſize, in order to contain the eggs they were to lay; and that the ſharp points of their tails were of a hard bony nature, and deſigned to penetrate into the moiſt earth and there depoſit their eggs, which otherwiſe, in my opinion, would be barren.

On my return home, when I examined the animals I had taken, I found ſix of them to be females and ten or twelve males, ſome of them were coupled together, which proved them both to be of the ſame ſpecies; and it ſeemed probable to me that the ſtrong winds, which had prevailed for ſome days paſt, had brought ſome of the females, which had not ſtrength to reſiſt them, into the town.

The next day moſt of theſe animals were dead, and I concluded that their deaths had been cauſed by my handling them too roughly, ſo as to break ſome of the many veſſels in their bodies.

Fig. 15, N O P Q, repreſents the female of this animal, and at N may be ſeen how ſharp pointed is the hind part of their bodies, to enable them to depoſit their eggs in the earth; this part is divided into four diſtinct parts, as joints, both above and below, which ſeparate and open themſelves.

The eggs of theſe creatures, of which I have ſaid that I took more than two hundred out of one parent, are very black, and of ſo ſmooth a ſurface that they ſhine like black glaſs; they are about twice as long as broad. The ſhell of them, in proportion to their ſize, is very hard and thick, and yet I always found that theſe eggs, whether laid by the animal, when confined, or taken out of their bodies by me, did, in a ſhort time, ſuffer ſuch an evaporation of their moiſture, that the ſhells were compreſſed inwards: which ſudden evaporation I have not obſerved in the eggs of much ſmaller inſects.

From hence I concluded that the conftitution of thefe creatures was fo ordained by Nature, that this Maggot, in the middle of Summer, when the atmofphere is very hot and the earth dry, does not undergo any change nor lay its eggs; for if thofe eggs were laid in hot and dry earth, they would be foon dried up and become barren, whereby the fpecies would be expofed to perifh. But, as thefe animals lay their eggs in the month of September, when our lands are all wetted with rains, the eggs, being laid in moift earth, do not dry up, but remain fruitful.

Having obferved that the animals I had put into a box in the beginning of September, had laid fome eggs in it; I did, in order to preferve them, put them into a box almoft filled with moift fand, and covered them with more of the fame, and carried them in my pocket for fourteen days, to give them warmth, and fee whether any Maggots would be produced from them, and I daily opened fome of thefe eggs, but could not fee any appearance of young within them.

I often opened the bodies of thefe animals, both the males and females, and was furprifed at the wonderful number of veffels and organs they contained, infomuch that I muft fay, that the fpectacle would excite more admiration than to view the inteftines of larger animals with the naked eye; and this was particularly the cafe in the hind parts of the bodies of both males and females, which, viewed through the microfcope, exhibited fuch a fpectacle, that I never faw the hind part of the body of any animal, wherein were fo many organs with their joints, the ufe of which, though doubtlefs effential to the animal, are to us unknown.

Since we fee now, as I have faid, that the female of this animal can lay more than two hundred eggs, it plainly appears, that if all the animals of this fpecies which are bred in one fummer were to increafe in the fame degree, within two or three years they would fo multiply, as to devour all the roots of our grafs; but by droughts in the earth, great rains and ftorms, and fevere frofts, many of

them are deſtroyed, and we are not infeſted with them equally every year.

From the generation of theſe creatures, we may make a tranſition to that of Locuſts, which are found in Eaſtern countries, and devour the fruits of the earth, and when they have no other food, devour one another. We do not often ſee Graſshoppers, (which are ſimilar to Locuſts) near this town, but I had at one time a parcel of large ones brought to me, which I put into a glaſs and fed with vine twigs, but obſerving that the large ones devoured the ſmaller, I ſeparated them, and found that the females laid above eighty eggs. Now, if we ſuppoſe, that Locuſts do the ſame in extenſive countries where the Maggots are not expoſed to tempeſts, heavy rains, or froſts, (and I am well aſſured, that Locuſts are produced from Maggots, from the obſervations I have made on Graſshoppers in this country, which are of the ſpecies of Locuſts), we may eaſily conceive that Locuſts in thoſe countries may multiply to that degree, that they may viſit the neighbouring countries in ſwarms. Theſe are merely my ſentiments on this ſubject, which, however, I think more conſonant to right reaſon, than the notions of thoſe who dream that Locuſts come out of the clouds for the puniſhment of mankind, as I have often heard in converſation.

On a Maggot which is found in the bloſſoms of fruit trees, particularly apples; the Fly from whence it is produced particularly deſcribed; with the Author's reaſoning on the production of minute flying animals.

ALTHOUGH it has always been my firm opinion, that no animal, however minute, is produced any otherwiſe than by courſe of generation, nevertheleſs, I determined to proſecute my inquiries in the examination of thoſe minute Flies and other inſects, which are found on the leaves of fruit-trees and plants in gardens.

Having heard frequent mention made of a certain black Fly, which was ſaid to be very noxious to the bloſſoms of fruit trees, I examined the bloſſoms on the fruit trees in my garden, and particularly the apple-trees, and found that many of the flowers did not blow out fully, nor were they of a white or pink colour, but appeared ſhrivelled and of a kind of ruſty red.

Upon opening theſe bloſſoms, I found within the leaves of moſt of them a Maggot of a pale yellow colour, and in ſome of them a cryſalis, which I concluded had been changed by tranſmutation, from one of thoſe Maggots; and as I, at the ſame time, obſerved many black Flies on the leaves and flowers of thoſe fruit trees, I concurred in the common opinion reſpecting them, and concluded that thoſe Flies had laid an egg on each flower, from which Maggots had been hatched, and that they, being changed into cryſales, did at length produce the ſame ſpecies of black Fly.

At the ſame time, I was of opinion, that if theſe Maggots were not depoſited within the bloſſoms before they were full blown, few of them would be preſerved alive, becauſe this Maggot is not incloſed in a web or caſe, as we obſerve in moſt other cryſales, and

therefore would be expoſed to be devoured by ſmall birds, or even by common ants; for I ſaw, that where an ant could find its way into the bloſſom, it had eaten up part of the Maggot or cryſalis. But theſe Maggots when encloſed within the bloſſom, do bite or gnaw the veſſels of its leaves in a manner, that not only the buds dry, but are cloſe ſhut up, and thus afford a defence to the Maggot during its growth, and until it is changed into a flying animalcule.

In theſe my obſervations, many things occurred to me worthy of note: firſt, that in all the bloſſoms I examined, I never found more than one Maggot in each flower; ſecondly, that from the ſmall nouriſhment it there received, it grew apace; and laſtly, that in a few days it became ſo perfect, as to be transformed into a cryſalis.

Many of theſe bloſſoms I pulled from the trees, concluding that no flower which contained a Maggot or a cryſalis, would ever produce any fruit. Placing ſome of theſe before the miſcroſcope, I perceived that the cryſalis and the young animal it would produce, had no affinity with the black Flies I have mentioned; and to be more certain of the fact, I put into a glaſs tube four of thoſe cryſales, which I carried about with me in my pocket, and at the end of five days, I perceived one of them had changed its pale yellow into a red or blackiſh colour, and ſoon afterwards it put off the ſkin which incloſed it, and ran about the glaſs: on the eighth day the three other cryſales put off their ſkins, but ſome of them were of a darker colour than others, having reſpect to the time they had been in their cryſalis ſtate.

I now perceived that, with many others, I was miſtaken, in ſuppoſing the blight, or rather the Maggots we perceive in the bloſſoms of fruit trees, to be produced by the black Flies; for theſe animalcules, I was now obſerving, had, as I before mentioned, no ſimilitude to the bodies of the black Flies; firſt, becauſe they were not nearly ſo large; and ſecondly, becauſe that which I found

within the bloſſoms, when changed from a Maggot into a flying animal, was provided with two ſhells or caſes, folded lengthwiſe over its wings, as we ſee inſects of the beetle tribe; moreover, the fore part of its head was furniſhed with a long proboſcis or trunk, deſigned by Nature, as I ſuppoſe, for perforating the buds of the bloſſoms before they were blown, and through the hole to introduce an egg, whereby the egg and the Maggot produced from it, might be protected from its enemies, particularly birds and piſmires; this proboſcis, about the middle of it, was provided with two horns.

I again viſited my garden, where many of the trees were yet in bloom, but neither on the trees, nor on the flowers, could I diſcover any of theſe flying inſects, which had been brought to their perfection while carried about in my pocket; though in ſome of the flowers which I opened, I found Maggots, and in others cryſales, yet not in one a flying animal; whence I concluded, that ſo ſoon as from a cryſalis they arrived at that ſtate, they forſook the bloſſoms and took their flight.

At the times when I took the Maggots I have before mentioned, out of the flowers, they twiſted and bent their bodies many ways; but I never ſaw them move progreſſively; for when I placed them upon a paper, they could not change their place, but rolled themſelves up in a kind of ring, for theſe creatures have no feet; therefore, each Maggot muſt be placed in a bloſſom by itſelf, and cannot injure more than one flower.

One thing ſeemed to me very remarkable, namely, that all the food which this Maggot took from the bloſſom (at leaſt, as far as I could perceive), became part of its body, for when I examined the flowers wherein the cryſalis had lain, I could never diſcover any appearance of excrements, but only the ſkin left behind by the animal upon its transformation.

In theſe my obſervations I could not but take notice how Nature, that provident parent, has created this animal ſo as not only to derive its ſupport and growth from ſo ſmall a portion of aliment, and in ſo little time, but alſo by appointing it to be placed within the bloſſoms

of fruit trees, and there brought to perfection for the confervation of its fpecies, otherwife (as before mentioned) it would be devoured by pifmires or birds: and it muft alfo be obferved, that thefe creatures have no ability of feeking their food at a diftance.

Befides what I have mentioned, I had others of thefe cryfales, which I kept in my ftudy to watch their transformation, placing them in a glafs, fo as to give them a fupply of air; but I never could perceive the leaft appearance of any evaporation from them, though, as foon as they were converted into living animals, fo great a quantity of moifture iffued from their bodies as to hinder their moving, and often to occafion their fticking on their backs to the glafs, which made it neceffary for me to give them more room: here is to be noted how wonderfully compact muft be the fkin of the cryfalis, fo as to fuffer no evaporation of its moifture, left the animal within fhould be injured by the want of it; and this is another inftance of the inconceivable correctnefs of Nature in her works.

Finding myfelf miftaken in opinion, refpecting the black Flies depofiting Maggots in the bloffoms, I determined, if poffible, to examine the nature of their propagation, and the rather as I affured myfelf that they muft be produced by cryfales. I therefore opened fome of thefe black Flies, and I can fafely fay that, from a rough calculation, I found in each female three hundred eggs.

Going into my garden in the morning, to take fome of thefe Flies, that I might confine them till they laid their eggs, and I might difcover what kind of cryfales would be thence produced, I perceived the Flies fitting on the leaves and flowers as immoveable as if they were dead, and even, upon a gentle touch of the branches, to fall to the ground; whereas all other Flies, in warm weather, are very nimble. Hereupon it occurred to me that, when the wind blows from the North, Flies do not rove about in the air, but fit upon the trees, where we obferve them in greater numbers than when the air is warm; and as in cold and cloudy weather they fettle in great numbers, not in the fummits of trees, but in the lower branches,

many people think they are brought to us by the North wind, or even bred in the clouds: but ſuch men I leave in their error.

Theſe black Flies (and eſpecially thoſe which I had ſuppoſed to be impregnated) being confined in two glaſſes, laid a great number of eggs, very long in proportion to their thickneſs; whence I gathered that they would be changed into very long and ſlender Maggots or Caterpillars. But as all the Flies ſoon afterwards died, and not one of the eggs produced a living creature, I conſidered that perhaps theſe Flies might die for want of food, and that the eggs might be laid before their time, having often obſerved flies and moths when I happened to handle them ſo roughly, that they afterwards died, immediately after ſuch handling, to empty themſelves of all the eggs they contained.

I did intend here to deſiſt from publiſhing any thing farther reſpecting the propagation of theſe inſects, deeming that I had proved ſufficiently, that all living creatures were produced by the ordinary courſe of generation; but my friends in different parts of the world, exhorted me to proceed in my diſquiſitions on this ſubject, as being what many learned men deſire may be proſecuted.

To proceed then in my examination of this ſubject. In the month of May, in the following year, the weather being warm, and my fruit trees in bloſſom, I ſought for ſome of the flying inſects I have mentioned, and preſently ſaw many of them coupling together. But though they were nimble in their motions, and well fitted to fly, and moreover had ſix feet, and on each foot two claws, yet on my ſhaking the leaves or branches ever ſo little, they gathered up their feet and fell to the ground; ſeveral of theſe ſmall animals I brought to my ſtudy in a glaſs, to make my obſervations on them.

I found the wings of theſe creatures to be about twice the length of their bodies: but, if we conſider that thoſe wings are covered with thouſands of hairs, and obſerve, that when the wing is ſpread, not the leaſt fold or joint can be diſcovered in it, though it lies on

the body folded together, we muſt be loſt in admiration, and the more, when we recollect the multitude of veſſels and joints, and alſo muſcles, requiſite for the formation and expanſion of ſuch a wing; and we may well aſk, whether any man in his ſenſes, who is not entirely blinded by prejudice, can contemplate ſuch a creature, by the microſcope, without acknowledging that it could not poſſibly be produced from corruption, or the putrefied bodies of other animals, and will he not rather cry out in the words of an eminent gentleman, who lately came to ſee my microſcopical objects, "O the depth of the Divine Wiſdom, how inſcrutable are "his works! Can any man after this, be found to deny the being "of a God?" or ſuch like expreſſions.

I cauſed a drawing to be made of this inſect, the ſame ſize as it appears to the naked eye, which is repreſented in Plate XVI. *fig.* 16, and to avoid giving a drawing of the whole animal when magnified, I cut off the head, and placed it before a microſcope to be delineated, in order hereby, to ſhew how the careful parent, Nature, has created this ſmall animal, that its ſpecies may be preſerved, and the order and perfection with which the world was at firſt created may not be diminiſhed.

Fig. 17, A I, repreſents part of the head: B H are the eyes, which, in like manner as the eyes of Flies, are formed of many optical organs, and appear with as many protuberances as the ſubſtance, called ſhagreen, appears to the naked eye.

C D E F G, is that organ or trunk, with which, as I ſuppoſe, the animal perforates the bloſſom, while yet a bud, in order to lay an egg in every bud; to which end, its extremity is provided with various piercers or teeth, and other weapons, which are, in ſome ſort, repreſented in the drawing, but the whole of which, the limner could not copy, becauſe the animal, while alive, was opening and ſhutting, or extending and contracting, theſe organs, and, when dead, they were almoſt out of ſight; and though I did all in my

power ſo to fix them, that they might remain viſible without the body, I could not ſucceed.

This organ C D E F G, is ſomewhat larger than it here appears, becauſe it is bent forward, and, being viewed from behind, the bend in it could not be deſcribed, therefore I gave the limner another microſcope, exhibiting a ſide view of the ſame, and a ſketch or outline of its curvature is to be ſeen at *fig.* 18, M N O.

D K and E L are the two horns with which this organ or trunk is furniſhed.

After this, I again looked over eight or ten of my apple trees, and, in the ſpace of half an hour, I collected thirty of theſe inſects, which I ſhook from the leaves into a large glaſs tube, that they might not be hurt by my touching them, and I might be able to obſerve how long they could live without food, and alſo, whether they would lay any eggs. While taking them, I obſerved, that not only, upon the leaſt motion, they drew up their feet and contracted their trunks and ſo fell down, but that ſome of them did the ſame upon being only touched by my hand, or by the glaſs.

But what I moſt wondered at was, that two third parts of theſe inſects which I found on the trees, were coupled together, and I could not conceive how ſuch ſmall animals could find each other out in ſo large a ſpace, and the rather, becauſe in a tree, the circumference of which was more than thirty-ſix feet, I only found four or ſix of them, but all in couples.

Obſerving that theſe inſects could run along or ſtand for a long time on any ſide of the glaſs, even with their feet upwards, I was deſirous to examine accurately the formation of their feet, and, in this little creature, I ſaw ſuch perfectly formed limbs, enabling it to adhere to the glaſs, and to run along upon its ſurface, as diſtinctly, as I had ever ſeen in other larger flying animals.

Fig. 19, P Q R S T, repreſents almoſt a fourth part of one of the legs, conſiſting of four diſtinct joints: the other part of the leg has only two joints. At R and S, are two claws or nails, which are

in ſome ſort tranſparent, like a piece of horn or tortoiſe-ſhell, ſeen with the naked eye, and at Q and T, are ſhewn the organs, by the help of which the animal can run along the ſmooth ſurface of the glaſs, and alſo hang to it a whole night. The formation of theſe organs is very wonderful, for all thoſe parts with which they are covered, and which one would conclude to be hairs, are ſo exactly and regularly ſloped off, and particularly of ſuch regular lengths, as if they had been all clipped with ſciſſars, that when the animal places its foot any where, they all touch the place at the ſame time, and what is more, all theſe particles, which ſeem to the eye to be hairs, have at their extremities a hook, and, at a little diſtance from thence, two other hooks; but by reaſon of their extreme minuteneſs, though the limner confeſſed he ſaw them through the microſcope, he declared he could not repreſent them in the drawing.

Now if we conſider, what I have always experienced, that a glaſs, though waſhed ever ſo clean, will have many particles adhering to it, though theſe are ſo ſmall, that the claws on the feet of ſmall flying inſects cannot take hold of them, we may eaſily conceive that theſe minute hooks, may take hold of the ſmall particles of water or motes from the air adhering to the glaſs. And here we may diſcover the error of thoſe who formerly ſuppoſed there were cavities in glaſs, wherein Flies could fix their claws and climb up.

In the ſpring ſeaſon I obſerved, on ſeveral parts of my apple trees, many caterpillars gathered together, which, in moving from place to place, faſtened themſelves by a kind of thread, and I began to conſider, whether theſe were produced by the black Flies, and it ſeems they are called by gardeners *de Wolf*. To ſatisfy myſelf in this reſpect, I cut off from the apple tree, two branches, on which were theſe caterpillars, placing the ends cut off in a veſſel of water in my hall, in order that the leaves might remain freſh, and afford nouriſhment to the caterpillars: when the leaves began to

wither, I placed freſh branches near the others, to which the caterpillars removed themſelves.

When theſe caterpillars came to their full growth, they prepared to encloſe themſelves in a web ſimilar to the ſilk worm, with this difference, that at one extremity of their web or caſe, there was a ſmall aperture. A repreſentation of this web is given at *fig.* 20. Theſe animals, thus incloſed in their web, I put into a glaſs tube, together with ſeveral others of the ſame kind which I had found on the apple trees, placing the whole in a large glaſs. After ſome time, I ſaw come out of the greateſt part of theſe webs certain white flying inſects, having their wings diverſified with black ſpots; they were ſomewhat ſimilar to thoſe flying animals found in granaries, and proceeding from the Maggot, which mealmen, and bakers likewiſe, call *de Wolf*, and of which I have treated fully in another place. I was deſirous of keeping theſe creatures alive till I ſhould ſee whether they would lay eggs, but they all died.

I have cauſed a drawing to be made of this flying animal, as it appeared to the naked eye, when come out of its web, and this is to be ſeen at *fig.* 21. To have made a drawing of it from the microſcope, would have been too troubleſome, by reaſon of the multitude of feathers with which, not only the wings, but the feet, the horns, and the whole body were covered, and alſo by reaſon of a crooked organ on the fore part of its head; the true form and make of which I was not able to obſerve with ſufficient accuracy.

Among theſe flying animals, I ſaw in the glaſs two blackiſh Flies, which I conceived muſt have been produced in this manner, namely, that ſome black Fly of the ſame ſpecies having laid its egg upon one of thoſe caterpillars, the Maggot thence produced had fed upon the caterpillar till it had acquired its full growth, and thereupon was changed into a black Fly, inſtead of the web producing a white flying animal with black ſpots.

In one of the glaſſes, wherein I had incloſed the webs I have mentioned, there iſſued from thoſe webs not only the flying animals

I have juſt deſcribed, but alſo a great number of Flies, ſo very minute, that I ſhould not have imagined they could exiſt in the open air, for that the heat muſt cauſe all the moiſture in their bodies to evaporate.

The ſight of theſe Flies cauſed me to open all the webs which had been left in the glaſs, and in one of them I found a great number of minute ſkins or caſes, from which thoſe Flies had proceded by tranſmutation from aurelias. From whence I concluded, that a minute Fly of this ſpecies, muſt have laid many eggs in the aperture of one of theſe webs, and the Maggots hatched from thence, muſt have fed upon the caterpillars in the web, until they came to their full growth, and then within the web be changed into thoſe minute Flies; otherwiſe, in my opinion, that ſpecies of Fly would become extinct. For we muſt lay it down as a truth, that many flying animals live on nothing but other living creatures, and for want of which food many of them die, eſpecially the ſmall ones, among which may properly be reckoned Flies: for if many Flies could not find particles of fleſh whereon to lay their eggs, the maggots bred from thoſe eggs muſt periſh.

This innate diſpoſition and foreſight in ſmall animals leading them to lay their eggs in thoſe places where the young maggots may find food and nouriſhment, will appear ſtrange to many. But if we conſider the nature of larger flying animals which are familiar to us, and that we never ſee geeſe, ducks, or ſwans, make their neſts in trees, or in fields far diſtant from the water, but always on the banks of ditches or rivers, becauſe they do not bring food to their young, who are by nature deſtined to ſeek it for themſelves; and therefore the parents, when the young are hatched, do no more than tend on them and protect them from enemies; whereas, on the contrary, we ſee that birds who are able to bring food to their young, build their neſts on the tops of trees and other high places, and that the young remain in the neſts, and do not endeavour to follow their parents, however hungry they may be, we ſhall ceaſe

to wonder, that an infect lays its eggs near the body of another, while in an aurelia ftate, where the young maggots may find nourifhment. And laftly, if we fee in quadrupeds, that many wild beafts have no other food than the bodies of thofe beafts which they devour, that there are many birds who feed only upon birds, and that the fame almoft univerfally obtains in fifhes, it will not appear ftrange to us, that among minute flying animals, fome feed on others.

I caufed a drawing to be made of this very minute fpecies of Fly, the fize it appears to the naked eye, which is fhewn at *fig.* 22, with a circle round it to make it more apparent; for I muft confefs, that when viewing it through the common fpectacles with which I write, I could not difcover it to be a Fly. At *fig.* 23, A B C D, I have given a reprefentation of one of the wings of this Fly, as feen through the microfcope, in order to fhew the wonderful formation of fo minute an animal. Thefe wings, which are four in number, are covered both on the edges and on their furface, with a great number of hairs, terminating in points, like thofe on our hands.

One of thefe minute Flies I placed before a microfcope of much lefs magnifying power than that from which the wing was drawn; by this we fee it to have two pretty horns on its head, each compofed of many joints, and every joint covered with hairs. And in contemplating the horns, I took notice of the eyes, wherein I plainly perceived many optical organs of which each eye confifted, as we obferve in larger Flies. All thefe objects are reprefented at *fig.* 24, E F G H I.

In contemplating fuch minute animals as this, and confidering that no part of them is made in vain, but that every one has its ufe, we fee ftill further reafon to admire the perfection of fo minute a creature. And when we fee the ftupendous wifdom of Nature's operations in the greateft, and in the leaft of her productions, we may well cry out again and again, " Away with the blind croakings

" of thoſe followers of Ariſtotle, who by their writings endeavour " to darken the truth, and to perſuade us that flying inſects or any " other animalcules can be produced from corruption !"

There is another ſpecies of minute Fly, which I believe lays its eggs in ditches, from which maggots are hatched, and thoſe maggots again become Flies of the ſame kind. I have not thought it worth while to purſue a minute inveſtigation, as to its being ſo propagated, becauſe the fact is, I think, already ſufficiently eſtabliſhed; but the feathers on its head, its eyes, horns, and wings, when viewed by the microſcope, are ſo wonderful to behold, that I have cauſed drawings to be made of ſome parts of it.

Fig. 25, is the Fly of the ſize it appears to the naked eye.

Fig. 26, is one of the wings viewed by the microſcope.

Now, if we conſider, not only the multitudes of hairs, as well round the edges of the wing, as in the other boney parts, which give it ſtrength; and alſo the incredible number of very minute hairs with which the membrane of the wing is covered, and which the limner, as near as he could, imitated in the drawing; and that all theſe cannot be compared with the great number of hairs with which the Fly's body and its feet are covered; and if we moreover conſider, that each hair is not formed of a ſingle veſſel, but of many, we muſt needs ſay, that there is a greater cauſe for admiration and reflection, in the contemplation of ſo ſmall inſignificant an animal, than in that of a horſe or an ox. And the deeper we endeavour to ſearch into the ſecrets of Nature, the leſs we are able to conceive the minuteneſs of the particles of which bodies are compoſed. And to give ſome idea of which minuteneſs, I have made theſe remarks on the wings of ſmall flying animals.

Fig. 27, repreſents one of the two horns on the head of this ſmall Fly.

And here we muſt ſee, that there is more wiſdom, perfection, and curious workmanſhip, in the formation of this ſmall Fly I have

been deſcribing, than of the large body of a horſe ; and we alſo muſt conclude, which I lay down for certain, that not only this ſpecies of Fly, but every living creature upon the earth, are by no means produced from any kind of corruption or putrefaction, but derive their origin from thoſe creatures which were made at the Beginning, or a very ſhort time after the parts, whereof our world conſiſts, were brought into exiſtence. Finally, the more we reflect on the conſummate wiſdom and ſkill of the Creator of the Univerſe, the leſs are we able to form adequate ideas of his Perfections.

Of a very noxious Animalcule, which in the ſpring infeſts the young ſhoots of fruit trees.

HAVING frequently obſerved the leaves at the ends of the young ſhoots of gooſeberry trees, and alſo on cherry and peach trees, to be very much contracted, and, as it were, rolled up, by which means the growth of thoſe trees was impeded; and perceiving at the ſame time, many ants on the leaves ſo contracted, I at firſt adopted the vulgar opinion, that the ants alone were the cauſe of that contraction in the leaves, and conſequently, of the impediment to the growth of the branches. In one year my cherry trees were ſo much infeſted with this contraction of the leaves, or rather by the Animalcules concealed in them, that, out of about thirty of thoſe trees, only two or three ſmall ones were unhurt.

I therefore determined to examine for what purpoſe the ants reſorted to thoſe young leaves, and what was the cauſe of the leaves being contracted; in doing which, I ſaw that the contracted leaves, and eſpecially thoſe on the gooſeberry trees, were covered with a great number of dark-coloured Animalcules, and that thoſe which were moſt full grown, and were about the ſize of a half grown louſe, were of a darker or blacker colour than the ſmaller ones. Theſe Animalcules were ſome of them ſo minute, as not to exceed the ſize of a common grain of ſand. Upon ſight of theſe creatures, I concluded that the ants reſorted to the contracted leaves for no other purpoſe, than to devour theſe Animalcules on them; and I was confirmed in my opinion, by ſeeing ſeveral, both of the ſmaller and larger ſort, to be almoſt wholly conſumed; ſo that nothing except their ſkins and feet remained.

I cut off a ſhoot from a cherry tree, which I brought into my houſe, in order to examine theſe Animalcules more narrowly, and the rather, becauſe moſt of the vermin which infeſt our fruit trees are, in their reſpective ſpecies, nearly of the ſame ſize, and are generated from caterpillars, which become creeping or flying animals; and, in like manner all ſorts of flies, known to me, are generated.

I again looked over many of the leaves, in order, if poſſible, to diſcover the eggs from which theſe Animalcules were produced, and the rather, becauſe I was certain, that their bodies were not compoſed of annular parts or rings, as is the caſe with flies, butterflies, and the like, which are produced from caterpillars, or maggots; and from thoſe rings or diviſions in their bodies, they have the name of inſects, among which the flea is alſo to be reckoned. But all my ſearch was to no purpoſe, at which I was much ſurpriſed, that among ſo many minute animals, not an egg was to be found. This ſeemed to favour the opinion of thoſe who will have it, that ſmall living creatures are produced ſpontaneouſly, but ſuch a notion appeared to me altogether impoſſible, though, at the ſame time, I was at a loſs how to inveſtigate the generation of theſe creatures. At length, I determined to open ſome of the largeſt of them, in hopes of finding eggs in their bodies; but, inſtead of eggs, I found, not without great admiration, young Animalcules in the bodies of the larger ones, and in ſhape ſo like the parent, that one drop of water cannot be more like another, and I extracted not a ſingle young one, but four, completely formed, from the ſame parent. Hereupon I judged, that it would be moſt expedient to cut off all the twigs and leaves of the peaches, cherries, and gooſeberry trees which were infeſted with theſe creatures, and throw them into the water to drown them, and try whether I ſhould not afterwards be leſs infeſted with theſe vermin.

Theſe diſcoveries led me to conſider, whether theſe Animalcules towards the end of ſummer, or when the leaves fall off, might not

lodge themſelves in the trunk or branches of the trees, to ſhelter themſelves there during the winter; and, in order to inveſtigate that matter, having a gooſeberry tree, which had been ſo infeſted with theſe animals, that it had grown very little during the ſummer, and I had therefore determined to root it up, I ſuffered it to ſtand till the 15th of the following January, when, after a long froſt and rain following, I cut a branch from the tree about a ſpan long, in a place where it was the thickneſs of my finger, and, examining it by the microſcope, I ſaw among the cracks in the bark, and among the ſmall dry leaves where the new bud was about to ſpring, and which fall off when the bud grows larger, and alſo among ſome fragments of moſs adhering to the branch, ſeveral of theſe Animalcules, which all ſeemed full grown. They were not only dead, but the hind parts of their bodies were perforated with a round hole, and their entrails gone, whence I gathered, that provident Nature had aſſigned theſe creatures their enemies, to prevent their ſpecies increaſing too faſt, and alſo for the ſuſtenance of other animals.

In this ſearch, I happened upon an Animalcule eight times ſmaller than the former ones, which moved its head, horns, and feet; and it alſo appeared to me, that it had been produced by tranſmutation from a maggot or caterpillar, becauſe I could plainly diſcover in it, thoſe rings or annular parts we obſerve in maggots. I alſo ſaw adhering to this branch a ſubſtance, which, upon more accurate inſpection, I took to be the web of ſome ſmall maggot, and on this were four eggs, the ſhape of birds' eggs, but no larger than ſmall grains of ſand, and which I thought might, perhaps, be the eggs of the before mentioned living Animalcule.

But what to me appeared moſt wonderful was, that I diſcovered two Animalcules, in ſhape like thoſe called by children lady-birds, but ſo minute, that a hair taken from my hand, was more than four times their thickneſs, and, upon comparing them with a grain of ſcowering ſand, it was, in my judgment, three

hundred and fifty times as large as either of them: one of thefe Animalcules was alive and moved its feet, and I was certain it was of full fize, becaufe it appeared to have been produced from a maggot: and I the more wondered at the fight of fo minute an animal, becaufe I could not have imagined fo fmall a creature could live in the open air; for, if we fuppofe the one half of this animal's body to confift of a thin or watery fubftance, one would conclude, fo fmall a quantity would be foon evaporated; but yet, when we find that fo fmall an Animalcule remains a long time alive, we muft conclude, that Nature, our kind and provident mother, has formed the fkin of this animal fo hard and folid, that little or no moifture can be evaporated from it.

Thefe were my obfervations during one fummer and the winter which followed. On the 17th of May in the following year, I did not find, among all my goofeberry trees, above three or four branches the leaves of which were contracted. Upon opening thofe leaves, I found eight or ten of the before mentioned Animalcules, and among them one of a dark colour, inclining to black, and larger than the reft. This, on account of its fize, I judged to be a female; and upon opening it, I took out of the body twenty-one young ones, of which one feemed to be completely formed, and of a lighter green colour than the parent; the others were of fuch different fizes and colours, that in the fmalleft, I could neither diftinguifh the eyes, nor the green colour.

I opened feveral other of thefe Animalcules, which I deemed to be females, and took out of their bodies many young ones; in fome more than thirty, and in none lefs than twenty, of different fizes, and fome fo fmall that I could neither diftinguifh their limbs nor eyes.

On the 20th of May I cut off three branches of this year's growth from a goofeberry tree, on which branches I was certain there was no animalcule of this fpecies; thefe I put into a veffel of water, and on the tops of two of them I put two and on the third branch three

of thoſe Animalcules, which I deemed by their ſize to be females, in order to ſee how ſoon, and to what degree, they would bring forth young: and, in twenty-four hours, two of theſe produced nine young ones and a third ſix; they continued to increaſe, but there being a veſſel, with flowers in it, brought out of the garden, which ſtood near that where the branches were, ſome ants, which were among the flowers, crept into the branches and killed ſome of the Animalcules, for I could plainly ſee that their bodies had been, in great part, devoured by the ants. Hereupon I killed all thoſe ants, and placed the veſſel containing the branches in a diſh full of water, to defend the acceſs to it from ants.

In theſe and others of my obſervations it appeared to me that our common ants, which I had always deemed very pernicious inſects, on account of the damage they do in our gardens, by devouring the fruit when ripe, do, on the contrary, in the ſpring, when there is no fruit, live upon other ſmall creatures; ſo that I am doubtful whether the damage they do is greater than the utility they are of in the ſpring.

Hereto I muſt add, that I have been at ſome times very much infeſted with ants, ſo that I cauſed them and their eggs to be dug up and thrown into the water: but afterwards my method was, where an ants' neſt was found, to cause its opening to be ſtrongly preſſed down with the foot, to deſtroy the ants in their neſt: and, if ants were found among the trees, to ſtamp the earth cloſe round the trees, in order to keep the ants within the ground, and prevent their bringing food to their young, whereby both would be deſtroyed, and by this means I almoſt wholly freed myſelf from them.

On the 30th of May I again examined the branches I had placed in my ſtudy, and found many young Animalcules on them, ſome of which were conſiderably grown; and I alſo perceived that it was in the nature of theſe creatures to change their ſkins, for I found ſeveral caſt ſkins, in which I could plainly ſee the feet, horns, hairs, and claws on the feet; and in theſe ſkins, which were very tranſ-

parent, I not only ſaw many veſſels, but I could diſcover many of the eyes, or optical organs, with which this creature is provided, all which were a pleaſant ſpectacle to behold.

Hitherto I had been of opinion that theſe Animalcules fed only on the veins or veſſels of the leaves, and that their breaking or biting them cauſed the leaves to contract, and under which contracted leaves the animals ſheltered themſelves from the heat of the ſun; but now I ſaw that they, for the moſt part, fed on the very ſmall buds and alſo on the ſtalks of the leaves, the veſſels of which being wounded cauſed the leaves to contract in their growth, and the nouriſhment from the young ſhoots being, in a great meaſure, taken away, the ſhoots grow knobbed and crooked.

Upon recollecting that, in the former ſummer, I had ſeen on my plums and apple trees a great number of flies which, as I thought, had ſo impeded their growth that not only the apples were very ſmall, but alſo the young ſhoots were very defective, I now determined to examine into the true cauſe of that appearance.

I could not, in my ſearch, diſcover any eggs of thoſe flies in my apples, peaches, and plum trees, but I now ſaw all the young ſhoots of one of my gooſeberry trees to be uncommonly contracted, and on it I not only ſaw a great number of thoſe Animalcules I have deſcribed, which breed their young within them, but alſo many black flies, whoſe bodies were not larger than the bodies of those Animalcules. I therefore pulled up the tree and threw it into the water, concluding it impoſſible to extirpate all the Animalcules and ſave the tree's growth, having firſt cut off two branches of it for further examination.

Theſe flies have four wings, the two largeſt of which are twice the ſize of their bodies; I could not, at firſt, think they were produced from the other Animalcules, but, upon examining them by the microſcope, I found the bodies of both of them to be very ſimilar, and, after ſeveral repeated obſervations, I ſaw that the firſt mentioned Animalcules had, on each ſide of their bodies, a long white

protuberant part, which, upon examination by the microfcope, I found to be wings. I alfo opened the bodies of feveral of the flies, and in them I found young ones of the fame form, and in like numbers, as the other Animalcules, all which plainly proved to me that thefe Animalcules, commonly called goofeberry lice, are changed into flies.

As I was affured that the Animalcules which had been bred in my houfe, and were now fourteen days old, had grown to their full fize, I opened three of them, and from the body of one of them I took about thirty young ones, of different fizes, from another forty-nine, and from the third fixty. At this very great increafe I was aftonifhed, and was defirous to examine all the goofeberry and other trees in my garden, in order to extirpate thefe noxious animals as much as poffible; and the rather, becaufe when changed into flies, they can fly into other trees, and fo infect the whole. But though we may clear our gardens ever fo much, they will be ftill liable to be infected from neighbouring ones.

Some of thefe Animalcules, whofe change I judged was near, I put into a glafs, in order, if poffible, to obferve the nature of fuch change; and, after a day or two, I faw one of them put off its fkin and affume a new form: its wings, which before had been folded up very clofe, it expanded by degrees, fhaking them flightly, and then they appeared as ftraight and regularly placed as if they had never been folded up, and the fame I obferved in others.

This kind of propagation appeared to me more wonderful than any I had before obferved; if we confider that fuch minute Animalcules, as thofe we are now confidering, fhall, within thirteen days after they are produced from the parent, breed within them fixty young ones, many of which can be feen to be completely formed; and moreover that thefe Animalcules, after iffuing from their parent, fhall, in their growth, many times change their fkins, produce a number of young, and laftly be changed into flying animals which continue to bring forth young; this muft appear wonderful, and be

a confirmation of the principle, that all living creatures deduce their origin from thofe which were formed at the Beginning.

And if we compute how many Animalcules one female may produce in the courfe of a fummer, reckoning from thofe we find in their bodies, without confidering thofe that efcape our notice, we muft be filled with aftonifhment.

Although thefe Animalcules were propagated fo rapidly in my houfe, as I have before fet down, I have found that in the open air they increafe ftill fafter.

I have alfo obferved another fort of thefe Animalcules, a little different from the former, for the bodies of the former were not only fomewhat broader, but, when I examined their heads and the hind part of their bodies by the microfcope, I found that thofe parts were alfo different: for the former, when full grown, had a dark colour, appearing blackifh; the latter were green, and fo were the flies produced from them. I alfo faw thefe two fpecies of Animalcules mix one among another, whence I concluded that they were the two fexes.

As foon as the young of thefe Animalcules come from the parent, they can creep about, and appear as vigorous, as if they had been long in the open air.

I have caufed drawings to be made of thefe Animalcules, in order to fhew the exact regularity and perfection which are fhewn in the formation of fo contemptible a creature.

Plate XVI. *fig* 28, reprefents one of thefe Animalcules of a light green colour, the fame fize it appeared to the naked eye. *Fig.* 29 A B C D E F G H, is the fame feen through the microfcope, being of its full growth and approaching to its change; the wings, folded together, begin to appear, and may be feen in the figure at W X. At K L M N, are the fix legs with their joints, covered with many very thin and fhort hairs, and each of the feet furnifhed with two claws. At F, is one of its two beautiful eyes, the wonderful formation of which, as it appeared through the microfcope, the limner

could not copy in the drawing. I I, ſhews the trunk, or piercer, which the animal ſtrikes into the bud, or ſtalk, on which it feeds: F Z Z, are the two horns.

In the hind part of its body this Animalcule has two parts, or organs, thicker in the middle than at the ends, which are curiouſly covered with round ſcales, placed in exact order beſide each other thereon, as ſhewn at C and B, and from theſe organs I often ſaw a ſmall drop of very tranſparent liquor iſſue, as ſhewn at B. This ſmall drop exhibited a very pleaſant ſpectacle to me, for, when removed a little further from the microſcope, it had the effect of another microſcope, ſhewing the objects, as houſes, ſteeples, and the like, inverted, and ſo minute and delicate in their appearance, that could ſcarcely be believed. When this drop of liquor was emitted from one of the organs, B or C, I ſaw that the organ from which it iſſued was immediately drawn in towards the animal's body.

D T U E, is the point of a needle, to which this animal was faſtened when the drawing was made.

Fig. 30, is the ſame Animalcule, of the ſize it was when changed into a fly.

While the limner was employed in making the above drawing, I diſſected ſome more of theſe Animalcules, and having taken out ſome of the unborn young ones, I placed the moſt perfect one of them before a microſcope, and cauſed a drawing to be made of it, which is ſhewn at *fig.* 31, O P: this animal's body appeared to be covered with a membrane, but the limner could not copy in his drawing all the parts which were to be ſeen, becauſe the moiſture of it in a very ſhort time evaporated, and thereby the ſhape was altered. I alſo gave the limner another microſcope, before which I had placed eight unborn Animalcules, in all which the eyes could plainly be ſeen, and of theſe he made as correct a drawing as he was able, which is ſhewn at *fig.* 32, Q R S.

In *fig.* 28, at F, I have ſhewn one of the curious eyes of this Animalcule, but meeting with one of the put-off ſkins, in which

the formation of it could be feen more accurately, I placed it, by itfelf, before a microfcope of greater magnifying power, and caufed a drawing of it to be made, in order to fhew the perfection exhibited in fo minute, and to us fo defpicable, a creature. *Fig.* 33, A B, reprefents this eye as feen in the put-off fkin, and it appears to be compofed of a great number of diftinct eyes or optical organs.

After this I opened the bodies of others of thefe Animalcules, in order to fee whether the young ones within them fhewed any figns of life, and I found one of the young Animalcules, in whofe body I could not only fee the motions of the inteftines, but alfo its feet to move, though they lay regularly placed on the fides of its body.

On the 6th or 7th of June, I faw that many of the Animalcules, which had been brought forth on the 21ft of May, were changed into flies, and fome of the leaves on the goofeberry branches whereon I kept them began to wither.

Until the 5th of June, I had often fought for thefe Animalcules on my cherry, peach, and plum trees, but found none: on the 7th of June, upon making a farther fearch, I found fome of them on fourteen different branches of the cherry trees; they were not green but blackifh, the young ones round about them were of a dark colour, and in one leaf I faw five Animalcules, full grown, which had one hundred young ones round about them.

I opened many of thefe Animalcules, and took many young ones out of them of the fame kind as before mentioned, but thefe laft young ones were of a dark colour. I alfo opened fome Animalcules which feemed to be half grown, and found in them many imperfectly formed young ones, and, to the beft of my judgment, I counted fixty in the body of one of them, befides which there were doubtlefs many which I could not difcern; the largeft of thefe were of a green colour.

I did intend here to have finifhed my obfervations, but feeing that thefe laft mentioned Animalcules were only on the cherry trees, and perceiving fome little differences in their formation from thofe I have before mentioned, I determined to try whether thefe laft

would feed on the gooſeberry tree; I therefore took three branches of this year's growth from a gooſeberry tree, which I put into a veſſel full of water, and put on each of them a cherry tree leaf, whereon were many of thoſe Animalcules, judging, that as the cherry leaves began to wither, they would leave them and remove to the gooſeberry leaves, which they accordingly did. But they did not remain cloſe to each other, as they had done on the cherry leaves, where they had food cloſe to them, but diſperſed themſelves among the gooſeberry leaves, creeping up and down among the branches. I then put two twigs of the cherry tree in the veſſel cloſe to the gooſeberry branches, and ſaw many of the Animalcules quit the gooſeberry and remove to the cherry branches; thoſe which did not remove to the cherry branches, I found afterwards dead; others got down to the water and there periſhed.

On the 23d of June, I was much ſurpriſed to ſee a great number of the Animalcules ſtill remaining on the cherry branches, and on the 26th, I ſaw ſome of them changed into flies; but what I moſt wondered at was, that ſome of the Animalcules, which I judged had lived through the whole winter, were not yet changed into flies.

I obſerved, among other things, that as ſoon as one of theſe Animalcules had put off its ſkin, and was changed into a fly, it immediately began to move its wings, whereby they opened a little from the ſides of their bodies, to which they before were cloſely attached, and in the ſpace of two minutes acquired their full ſize; and, at this time, I thought I ſaw the parts of the wings not only unfold, but alſo encreaſe in ſize.

Moreover, I opened ſome of the Animalcules which were changed into flies, and took ſeveral young ones out of their bodies; theſe animals had alſo a piercer, which they thruſt out of the organ, *fig.* 28, I I, and which is there ſhewn at I A: this is put off with the ſkin when they undergo their change.

Another circumftance appeared to me particularly worthy of note, that I faw about fifteen of thefe Animalcules, whofe bodies were fo fwelled, that they feemed almoft globular: fome of them were dead, others moved but little, and upon opening them, I found their fkins very tough, compared with others of the fpecies, and on the infides I found no appearance of any young nor any bowels, but only a white thick maggot, which took up almoft all the fubftance of their bodies. I put twelve of thefe, fome of them living, others dead, into a glafs, and in a little time they were all dead. But after eight days, namely the 24th of June, I opened one of them, and took out of it a living maggot, which lived for the fpace of three days in the glafs wherein I put it: this maggot appeared to me, like thofe produced from the eggs of ants; for like thofe maggots, it feemed incapable of moving from place to place, as it had no other motion than what confifted in extending and contracting its body in a fmall degree.

I was aftonifhed at this fpectacle, refpecting which I could form no other conjecture, than that thefe Animalcules had been impregnated by a female ant laying its egg in the hind part of the animal's body, which is the only way I could conceive that the maggot from an ant's egg could be found in that place.

I opened feveral others of thefe Animalcules, at different times, and found maggots in all of them; but I could not, in any inftance, extract the maggot without injuring it. I left fome of thefe Animalcules which were dead in the glafs, and after a few days, I found that the maggots within them were dried up.

On the 26th of June, I found on a plum tree, on which I could not for three days before difcover any, a great number of green Animalcules, the largeft of them about the fize of a common loufe, and with which fome of the leaves were fo covered, that their furface could not be feen, and this for the moft part by Animalcules but lately brought forth.

I took a leaf on which were only two large ones, and two or three young ones, ſuppoſing that the full grown ones had not yet brought forth many young; one of theſe I opened, in order, if poſſible, to compute the number of young I might take out of it, and I thought that the young which I could diſtinguiſh, and thoſe particles which I conſidered to be young ones yet unformed, exceeded the number of ſeventy.

At the ſame time, I ſaw, among many others, ſix Animalcules changed into flies, in all which I found, upon opening them, a great number of Animalcules, and, among the reſt, one that was upon the point of being voided by the parent; and I not only ſaw it move, but I could ſee the inteſtines within it move about as vehemently as if it had been a parcel of living creatures.

But what ſeemed to me the moſt extraordinary of all my obſervations was this: that in every one of the Animalcules which I opened, though but of a middling ſize, I found young ones, and alſo upon opening ſome whoſe bodies were very ſlender, and which, for that reaſon, I thought might be males, yet among theſe and all others of the Animalcules, I have been deſcribing under this head, I could not find any that could be deemed males.

Farther, I opened ſome Animalcules, which were about an eighth part the ſize of the parent ones, and I took out of them a great number of round and pellucid particles, which, I doubted not, would in time have become perfect young ones; ſome of the largeſt of theſe contained ſome green particles, and moreover, they were of different ſizes, the ſmalleſt of them appearing no larger through the microſcope than a grain of ſand viewed by the naked eye.

I afterwards opened ſome Animalcules twenty-four times ſmaller than the full grown ones, and in them I alſo ſaw ſome of thoſe round particles, which I concluded would in time become young ones; and finally, I opened one, which was not much larger than

thofe newly voided by the parent, and, in my opinion, was only one day old; but I was not able to judge with certainty, refpecting the particles which I took out of its body, (by reafon of their exceffive minutenefs), whether they were fuch as would in time become young animals.

Addition, by the Translator.

THE Tranflator hopes to be indulged in bearing his teftimony to the accuracy of Mr. Leeuwenhoek's inveftigation of the preceding fubject, and alfo as to the utility of his difcoveries.

This part of the tranflation having been made at the houfe of a friend in the country, and in the month of June, the Tranflator was led to examine the fruit trees in the garden, expecting to find fome of the Animalcules mentioned in this Effay: he faw many of them exactly agreeing with the defcription here given; on the goofeberry and currant trees (particularly the latter) they faftened themfelves at the extremities of the young fhoots, and the farther extenfion of the fhoot feemed to be entirely prevented, for the bud in the centre was deftroyed and the ftalk of the fhoot grown into an irregular knobbed form, and fwelled to two or three times its natural fize, having the fame appearance as a gouty limb.

Some perfons may perhaps be difpofed to ridicule the great pains taken by Mr. Leeuwenhoek to inveftigate the nature of fo minute, and, as they may think, fo contemptible a creature, as that under confideration. But the ftate of the fruit trees juft mentioned will at once demonftrate that thefe minute animals are of a formidable nature, and capable of deftroying all the next year's produce. For if they attack the young fhoots in the early part of their growth, and thereby put a ftop to their further vegetation, the confequence muft be that from thofe trees, whofe fruit is produced upon the laft year's wood, little or no fruit can be expected in the fucceeding year.

On certain Animalcules found in the ſediment in gutters on the roofs of houſes. *

I HAVE been induced to publiſh my diſcoveries reſpecting theſe creatures, in order to ſhew how wonderfully Nature has provided for the preſervation of their ſpecies.

On the 25th of Auguſt, I ſaw in a leaden gutter at the fore part of my houſe, for the length of about five feet, and the breadth of ſeven inches, a ſettlement of rain water, which appeared of a red colour; and, upon conſidering that perhaps this colour might proceed from ſome red Animalcules, ſimilar to thoſe which I had ſeen in muddy ditches, I took a drop of this water, which I placed before the microſcope, and in it I diſcovered a great number of Animalcules, ſome of them red, and others of them green. The largeſt of theſe, viewed through the microſcope, did not appear bigger than a large grain of ſand to the naked eye; the ſize of the others was gradually leſs and leſs: they were, for the moſt part, of a round ſhape; and in the green ones, the middle part of their bodies was of a yellowiſh colour. Their bodies ſeemed compoſed of particles of an oval ſhape; they were alſo provided with certain ſhort and ſlender organs or limbs, which were protruded a little way out of their bodies, by means of which they cauſed a kind of circular motion and current in the water: when they were at reſt and fixed themſelves to the glaſs, they had the ſhape of a pear with a ſhort ſtalk. Upon more carefully examining this ſtalk, or rather this tail, I found that the extremity of it was divided into two parts,

* This ſpecies of Animalcule is very minutely deſcribed by Mr. Baker, in his Treatiſes on the Microſcope, and he has given to it the name of the wheel animal: alſo in Adams on the Microſcope.

and by the help of thefe tails the Animalcules fixed themfelves to the glafs; the leffer of thefe appeared to me to be the offspring of the larger ones.

Moreover, I faw another kind of Animalcules much fmaller, the bodies of them were very tranfparent; but there feemed to be an hundred of the former fpecies to one of them.

On the 31ft of Auguft, the water which I had before obferved, was, by three fucceffive days of hot weather, fo dried away, that when I preffed my finger on the muddy fediment in the gutter, no more water than about the fize of a grain of fand adhered to my finger, in which water I difcovered a fmall number of tranfparent living Animalcules, but all the green and red ones were dead.

The firft of September the fediment in this leaden gutter was fo thickened, that it appeared like ftiff moift clay; but, with all my endeavours, I could not difcover any Animalcules in it of the fpecies I had before feen.

At length I difcovered two living Animalcules with oblong bodies, like the largeft of thofe which I had formerly feen in rain water, wherein pepper or ginger had been infufed. Thefe Animalcules were almoft the thicknefs of a hair of one's head; but fuch of them whofe bodies were full of young, were twice that fize, the ends of their bodies terminating in a point; their tails were provided with fix or eight minute organs, by the help of which they could faften themfelves to the glafs, and the fore part of their bodies being alfo provided with certain organs, when they would move from place to place, they brought their hind parts nearer to the fore part, and then, loofing the fore part, they extended it in like manner as we fee caterpillars do; and, in fwimming, they made ufe of other organs deftined for that purpofe. Soon afterwards I obferved many of the fame fpecies of Animalcules.

The matter in the bowels of thefe creatures was for the moft part red, proceeding (as I imagine) from their feeding on fmaller Ani-

malcules of that colour, but fome few of them had no red colour in them, efpecially the fmaller ones, which probably had not been long brought forth from the parent.

On the fame day the weather was very hot, and, in the afternoon, I took a fmall part of the fediment from the gutter which was now quite dry, and I faw the furface of it completely red, by reafon of the great number of red Animalcules in it, being many more than the green ones; but I could not diftinguifh them until I had moiftened the fubftance with fome rain water.

The following day the fky was again very hot and dry, and, about nine in the morning, I took fome of the fediment which had been in the leaden gutter, which was then quite dried, and no thicker than half the back of a knife; it had alfo lain from the preceding evening in my ftudy; this I put into a glafs tube, about the thicknefs of a fwan's quill, and poured on it a fmall quantity of rain water taken out of my ftone ciftern, in which water were fwimming fome of the before mentioned Animalcules of the fmaller fort; having poured in this water, I mixed it up with the dry fediment or matter put into the tube, and which feemed very hard and compact, in order to diffolve the fame; that thus, if there were ftill any living Animalcules in it, they might iffue forth; though I confefs I never thought that there could be any living creature in a fubftance fo dried as this was.

I was, however, miftaken; for fcarce an hour had elapfed, when I faw, at leaft, an hundred of the Animalcules before defcribed; fome of them adhering to the glafs, fome creeping along upon it, and fome fwimming about. In the evening I computed there were more than three hundred of the fame kind of Animalcules, but the moft of them were not of full fize, as I judged by their bodies being fo minute, and fo empty of food, as if they were newly born; and in the bodies of fome of the larger ones, I could fee two, in others three, young ones, folded double: thefe young ones, when

newly born, were as quick in their motions as the full grown ones.

In that part of theſe Animalcules which may be called the breaſt, I ſaw a round particle moving with a reciprocal contraction and dilatation, in the time one might count one: this I did not doubt was the heart.

Moreover, the fore part of the bodies of theſe Animalcules, which may be called the head, was divided into two parts, each of the two diviſions being of a round ſhape, ſet round with certain long and very ſlender organs, which, in their motion, exhibited a moſt pleaſing and delightful ſpectacle; to form an idea of which, we muſt ſuppoſe we ſee two ſmall wheels ſet round the edges with ſharp points or pins, and theſe wheels in ſwift motion, as it were, from the Weſt towards the South and Eaſt, but never to move in a contrary direction from the Weſt towards the North and Eaſt.

This ſpectacle appeared to me the more wonderful and incomprehenſible, becauſe it is not to be conceived how ſuch a motion can be produced or performed in an animal body.

In order to give a clearer conception of this, I took a glaſs tube with ſome of theſe Animalcules in rain water, which I placed before the microſcope, and delivered the ſame to the limner, that he might make as exact a drawing of it as he was able.

Plate XVI. *fig*. 33, A B C D E F G repreſents one of theſe Animalcules, which had fixed itſelf to the glaſs by the help of the limbs or organs at A. The round oblong particle, appearing between B and G, I take to be a portion of its food nearly digeſted; that which is ſituated in the middle, I think is the food in the ſtomach and inteſtines; and the four oblong particles which ſeem, as it were, to ſurround the inteſtines, exhibit the young Animalcules in the body of the parent.

Between the letters D and E, appear the two round parts, ſhaped like wheels, with ſharp points placed upright on them. Theſe

wheels moving from the point D, which we will call the Weſt, and carried round, with a very ſwift motion, by the South towards the point E, which we will call the Eaſt.

When one of theſe Animalcules is creeping along the glaſs, it aſſumes the figure H I K L M N O, *fig.* 34: the parts, H and M, being alternately fixed to the glaſs, and in this ſituation the organs, like wheels, which in the former figure are ſhewn between D and E, are drawn within the body, and a part like an horn, marked L, appears in ſight. Beſides theſe poſitions, the animal twiſts and turns its body into ſuch wonderful ſhapes, that I have often been aſtoniſhed to behold it.

The limner alſo ſaw ſeveral Animalcules of a ſhape ſimilar to what is repreſented at *fig.* 35, P Q; theſe had the lower parts of their bodies of a flat ſhape, from which iſſued various minute organs, which they uſed in moving themſelves from place to place. Now in the body of this Animalcule, were many globular particles, and in the ſame water were many ſtill ſmaller Animalcules, whoſe whole bodies appeared no larger through the microſcope than one of thoſe globules in the former Animalcule.

I have often placed the Animalcules I have before deſcribed out of the water, not leaving the quantity of a grain of ſand adjoining to them, in order to ſee whether, when all the water about them was evaporated and they were expoſed to the air, their bodies would burſt, as I had often ſeen in other Animalcules. But now I found that when almoſt all the water was evaporated, ſo that the creature could no longer be covered with water, nor move itſelf as uſual, it then contracted itſelf into an oval figure, and in that ſtate it remained, nor could I perceive that the moiſture evaporated from its body, for it preſerved its oval and round ſhape unhurt.

In order more fully to ſatisfy myſelf in this reſpect, on the third of September, about ſeven in the morning, I took ſome of this dry ſediment, which I had taken out of the leaden gutter and had ſtood almoſt two days in my ſtudy, and put a little of it into two ſeparate

glafs tubes, wherein I poured fome rain water which had been boiled and afterwards cooled.

This fediment confifted of a fmall portion of earth, fome fand, pieces of mortar; and among it were mixed fome pieces of hair, threads of wool of different colours, and bits of ftraw, which things we may fuppofe to have been brought thither by the winds; and the furface of it confifted of thofe red and green Animalcules, apparently dead.

As foon as I had poured on the water, I ftirred the whole about, that the fediment which, by means of the hairs in it, feemed to adhere like a folid body, might be the fooner mixed with the water: and when it had fettled to the bottom of the glafs, I examined it, and perceived fome of the Animalcules lying clofely heaped together. In a fhort time afterwards they began to extend their bodies, and in half an hour at leaft an hundred of them were fwimming about the glafs, though the whole of the fediment which I had put into it did not, in my judgment, exceed the weight of two grains. After five or fix hours had elapfed I faw two feveral forts of Animalcules fwimming in the water, the leaft of which were fo minute, that many thoufands of them would fcarcely equal the fize of a grain of fand.

The preceding experiment I afterwards repeated, and met with the fame event.

Thus we fee that thefe Animalcules, when the water dries away, contract their bodies into an oval fhape, and, even in the heat of fummer, preferve their oval fhape for a long time: and, that when they are again fupplied with water, they, in a very little time, unfold and extend their different limbs or organs, ufing them in the fame manner, and with the like motions as they did before the want of water caufed them to contract themfelves. And this I obferved, not only in the full grown ones, but in the moft minute of thofe Animalcules.

Hence we may conclude, that in like manner as the fhells of the eggs of moths or butterflies, whence caterpillars are hatched,

and alſo the membranes of grubs or cryſales produced from caterpillars, are of ſuch ſolidity and firmneſs that the moiſture in them will not evaporate, ſo the ſkins of the Animalcules I am now treating of are formed of ſuch a ſolid texture, that they do not permit the leaſt evaporation: and, were it not ſo, I will venture to affirm that theſe creatures in very dry weather, being deprived of water, muſt all periſh: and this, which we find to be the caſe with theſe, we may fairly conclude takes place with Animalcules a thouſand times leſs.

We can now eaſily conceive, that in all rain water which is collected from gutters in ciſterns, and in all waters expoſed to the air, Animalcules may be found, for they may be carried thither with the particles of duſt blown about by the winds. And not only ſo, but Animalcules, millions of times ſmaller than a grain of ſand, may be carried up in particles of water, if not to the clouds, yet to ſuch a height as to deſcend with the evening dew: or by the winds they may be raiſed from the earth, and ſpread on all ſides. Nor will this appear incredible, if we conſider that, in great ſtorms, the ſea water is daſhed with ſuch force againſt the ſhore, that the particles of it are carried many miles, ſo as by ſettling on the leaves of trees and herbs to give them a ſalt taſte.

The preceding kinds of experiments I have many times repeated with the ſame ſucceſs, and in particular with ſome of this ſediment, which had been kept in my ſtudy for above five months, and upon pouring on it rain water, which had been boiled and afterwards cooled, I ſaw in a few hours time many of the Animalcules before deſcribed. And if, after being ſo long in a dry ſtate, theſe Animalcules, upon water being given to them, can unfold their bodies and move about in their uſual manner, we may conclude, that in many places, where in ſummer time the waters ſtagnate, and at length dry up, there may be many kinds of Animalcules, which, though not originally in thoſe waters, may be carried thither by water fowl, in the water or mud adhering to their feet or feathers.

From all thefe obfervations, moft plainly we difcern the incomprehenfible perfection, the exact order, and the infcrutable providential care with which the moft wife Creator and Lord of the Univerfe, has formed the bodies of thefe Animalcules, which are fo minute as to efcape our fight, to the end, that the different fpecies of them may be preferved in exiftence. And this moft wonderful difpofition of Nature with regard to thefe Animalcules, for the prefervation of their fpecies; at the fame time that it ftrikes us with aftonifhment, muft furely convince all of the abfurdity of thofe old opinions, that living creatures can be produced from corruption or putrefaction.

On the circulation of the blood in the tail of an Eel, and in the tails and fins of other fifhes: with a particular defcription of the Author's apparatus for viewing it.

AMONG * other objects, wherein I have viewed the circulation of the blood, was an Eel, and in the fin of its tail, I faw the blood circulating through an incredible number of arteries and veins of various fizes; and although thofe blood-veffels were difperfed promifcuoufly one among another, yet I could very plainly diftinguifh the arteries and veins.

Upon fixing my eye almoft at the extremity of the tail fin, I there faw fuch a number of minute blood-veffels, that it was impoffible for me to difcern the circulation in the minuteft of thofe veffels. This fin being a little dirty, I took a piece of linen cloth, and gently wiped it twice, to get a better view; but I found that in that flight touch, many of the external blood veffels were fo injured, that many particles of blood iffued forth, caufing the extreme parts of the fin to appear red, and they were alfo a little diftorted: I alfo plainly faw in all the arteries, however minute, a ftrong and quick fucceffive elevation, with a protrufion of the blood; and, upon attending to a large artery in the tail, I there faw the protrufion of blood, derived from the heart, to be much more vehement.

I alfo placed before my fight one of the two fins which the Eel has next its head, and there I faw the blood circulating in as many diftinct places as I have juft mentioned refpecting the tail fin. And when I endeavoured to purfue the circulation to the extremity of this

* See more on this fubject, in Vol. I. p. 89. *et seq.*

fin, I there ſaw its motion, both in arteries and veins, in ſuch an inconceivable number of exceſſively minute veſſels, that it looked as if the fin in that part of it was compoſed of nothing but blood veſſels.

In this and many other Eels, I ſaw that the blood veſſels, where the circulation comes to its full extent at the extremity of the tail fin, turned back with a ſmall bending, many of them taking their courſe tranſverſely, or croſſing one another, in their return back towards the heart. For inſtance, the circulation appeared to me as in Plate XVII. *fig.* 1, A B C D E, which repreſents a blood-veſſel ſituated between two of thoſe ſmall bones which give ſtrength to the extremity of the tail fin. In this figure, A B C is to be called an artery, becauſe in it the blood moves forward from A by B towards C, and from C by D, it returns to E; therefore C D E muſt be called a vein, though each of theſe veſſels, ſo called, is but one and the ſame blood-veſſel prolonged; and in its return it twice croſſes itſelf. Near to this was one of the ſmall bones of the fin, and cloſe to it a ſecond blood-veſſel, in which alſo the circulation forward and backwards was performed; and this veſſel in its return alſo twice croſſed itſelf, but with ſomewhat ſmaller bendings than in the other veſſel. Here, F G H, is an artery, wherein the blood was carried forward as far as H, and, bringing back the blood towards the heart, in the direction H I K, it is then to be called a vein. On this occaſion I ſaw, (and which I had obſerved in ſeveral other inſtances) that in the firſt mentioned artery A B C, there aroſe, at C, a ſmall veſſel which conveyed the blood from A B C, into the other veſſel at I.

To give an idea of the relative ſize of theſe veſſels, and of their diſtance from each other, the reader will underſtand, that the diſtance from E to K was almoſt equal to the breadth of four hairs of one's head.

To make theſe obſervations, my method at firſt was, to wrap the Eel in a piece of paper, or a cloth, leaving out only that part of

the fin which was to be examined; but, with all my care, I found that, in doing this, I either impeded the circulation or injured the fin; I therefore adopted this method: to take Eels about the ſize of one's finger, and let them ſwim about in the water for a time, till they were at reſt, and then examine their tail fins while in the water, for in that ſituation they ſpread out their tails, but when taken out of the water, the very extremity of the fin gathers up in wrinkles.

In order to impart this pleaſing ſight to others, I invented and compleated ſome inſtruments, whereby I exhibited the ſame to ſeveral gentlemen of eminence in this country, who all declared, that they did not think any more delightful ſpectacle, nor more worthy of obſervation could be ſhewn.

I have determined to give drawings of the inſtruments by which I exhibited this circulation of the blood, and by which thoſe who deſire to view the ſame may be eaſily gratified.

I firſt prepare a piece of braſs about half the thickneſs of the back of a common knife, and of the ſhape repreſented at *fig.* 2, A B C D E F; this I hammer upon an anvil ſome time, to make it the tougher, and make two large holes in it, as ſhewn at G H I and K L M. I alſo drill ſix ſmaller holes at P Q R S and N O. I then bend this piece of braſs at the end A B F, and at the dotted line B F, ſo that it may form a right angle with the other part, and alſo bend the other end C D E, in like manner.

I then take another flat piece of braſs of the ſame thickneſs, which, being well hammered, will anſwer the purpoſe of a ſpring; this is repreſented at *fig.* 3, A B C D, and I hammer it into ſomething of a hollow ſhape, and file in the lower end of it a cavity, in order that it may in part ſurround the glaſs tube I ſhall hereafter deſcribe; this round cavity is ſhewn at A D. I then drill four holes in it at E F G H, to correſpond with thoſe marked P Q R S in *fig.* 2; and with braſs rivets I join the two pieces together. I then prepare another braſs ſpring, as at *fig.* 4, I K L M, in which

I alſo make a hollow, as at K L, to embrace the glaſs tube, and having drilled two holes in it at A B, to correſpond with the two holes Q R, in *fig.* 2, I place it on the contrary ſide of the plate, *fig* 2, and rivet it with two braſs rivets through the holes Q R.

I then prepare another piece of braſs more than twice the thickneſs of the former, to be ſcrewed cloſe to the plate, *fig.* 2, and to which the microſcope is to be fixed; this piece of braſs is repreſented at *fig.* 5, N O P Q R S T, and in this I drill two holes to correſpond with thoſe at O and N in *fig* 2; theſe holes, V and W, are made to receive ſcrews in them, in order to fix by them the plate, *fig.* 5, to the plate, *fig.* 2. In this piece of braſs, I alſo drill a third hole with a ſcrew fitted to it, by which the magnifier may be adjuſted at a proper diſtance, which hole is ſhewn at X. I make likewiſe in this piece of braſs a fourth hole, rather larger than the former, in order that the magnifier, when adjuſted by the ſcrew, may be moved to the one ſide or the other: laſtly, I bend this piece of braſs, at the place marked with the dotted line PR, to a right angle.

I then make three ſcrews, one of which is ſhewn at A B, *fig.* 6, and with two of them I ſcrew the braſs, *fig.* 5, to the plate, *fig.* 2, paſſing one ſcrew through the hole O, *fig* 2, and V, *fig.* 5, and the other through the holes N, *fig.* 2, and W, *fig.* 5: the third ſcrew is to fix the magnifier by the hole Q, to the piece of braſs, *fig.* 5. I alſo make a fourth ſcrew, as at *fig.* 7, C D; the only uſe of which is, that, being paſſed through the hole X, in *fig.* 5, it may ſerve to fix the magnifier at a proper diſtance from the object to be viewed.

Fig. 8, E F G H I, are two thin plates of braſs or ſilver joined with five rivets, between which the magnifying glaſs is placed at L; and at K is ſhewn a hole made to receive a ſcrew, by which the magnifier is fixed to the braſs inſtrument before deſcribed.

I have alſo cauſed a drawing to be made of the whole inſtrument, as viewed on one ſide, (before the magnifying glaſs is joined to it),

which is ſhewn at *fig.* 9, and in this figure L M N T, ſhews that part which, in *fig.* 2, is repreſented at A B F: and the round hole in that figure is in this ſhewn at a b c. In this figure alſo O P Q R is the part repreſented in *fig.* 2, at C D E, and the round hole, d e f, in this figure, is the hole K L M, in *fig.* 2. The braſs ſpring V W X, is that which, in *fig.* 3, is ſhewn at A B C D, and the ſpring A B C D, in this figure, the ſame which is ſhewn, in *fig.* 4, at I K L M. The piece of braſs ſhewn in *fig.* 5, N O P Q R S T, is repreſented in this figure at E F G, and the two ſcrews, by which it is made faſt, are ſhewn at Y and Z; the braſs ſcrew, in *fig.* 7, marked C D, is in this figure ſhewn at H, where it is ſeen ſcrewed in its place.

I then directed the limner to make a drawing of the entire inſtrument, with the microſcope joined to it, and the glaſs tube incloſed in it, the ſame being adjuſted ready for uſe, and ſo placed before the view; this is ſhewn at *fig* 10, A B C D E F G H; the ſilver or braſs plates, incloſing the magnifying glaſs, are ſhewn at I K L M; the braſs ſcrew, by which the magnifier is fixed, is ſhewn at 2; the ſcrew, by which the object is placed at a proper diſtance, is marked at 3, and the glaſs tube, containing the Eel, and fixed in the inſtrument, is repreſented by Q R S T V W X Y.

It is my practice to uſe glaſs tubes of a ſize ſuitable to the Eels which are to be viewed; and though but a ſmall Eel be placed within the tube with its head towards the bottom, yet the glaſs tube will project ſo far below the inſtrument, that it may be held in the hand by that part, ſo that it will not be neceſſary to touch the inſtrument with the hand, and with the other hand the magnifier, which will be held next the eye, may, by means of the ſcrew at 3, be drawn out, or compreſſed inwards, until the part of the object to be viewed is brought to a proper diſtance.

When I wiſh to take out or put in the glaſs tube, which I generally do at the upper end, I remove the magnifier a little on one ſide, that it may not be ſcratched by the glaſs tube; this poſition or removal is marked in the figure by the dotted line N O P F.

I alſo made another inſtrument of like ſhape with the former, in which the ſpring repreſented in *fig.* 9, at A B C D, is a little ſhorter; and to this I ſcrewed the braſs containing the magnifying glaſs, as repreſented in *fig.* 11, H I K L, on which magnifier or microſcope, I fixed a little diſh or concave reflector, to throw more light on the object; to which intent I filed away as much as I could of the braſs about the microſcope, as may be ſeen at *fig* 12, M N O P, where this is ſhewn on the oppoſite ſide.

I have alſo given a drawing of the glaſs tube containing the Eel, which is ſhewn at *fig.* 13, Q R S T V. The Eel placed in this tube, I at firſt uſed to cover entirely with water, but I found that when the tail was left out of the water it would, being wet, ſpread itſelf on the glaſs, and be more eaſily viewed (this fin of the tail is ſhewn at W); and I always, before putting in the Eel, wetted the inſide of the glaſs, for otherwiſe the glutinous matter from its body would adhere to the dry glaſs and impede the view.

I alſo recommend to thoſe who make theſe experiments, as ſoon as ever the Eel is taken out of the tube, to let the glaſs be well cleaned, for otherwiſe the glutinous matter from the fiſh will dry on the glaſs.

I have cauſed many glaſs tubes to be blown for theſe experiments, but the rounder they are, and the thinner the glaſs, the better: ſome of them I cauſed to be made cloſed at one end, as here ſhewn at Q, others I cloſed with a cork.

I have alſo viewed the circulation of the blood in the tail fin of a Perch, and that in ſuch minute veſſels as would only admit one globule or particle of blood to paſs through at a time.

I once ſaw in a large artery in a Perch, (I call it large in reſpect of the moſt minute ones, though this was not thicker than an hair of one's head), and which is repreſented at *fig.* 14, the blood protruded from A to B, though but faintly; from B to C, I could only perceive a ſmall agitation of the blood, and beyond C it was coagulated: from this artery there aroſe a branch as repreſented in the

figure at B E, which carried almoſt all the blood from A B, through B E, into the vein D E F, in which vein the blood was conveyed from E to D, and all the blood which lay between E F was coagulated.

When thoſe Perch which I examined were very lively, and their tails unhurt, I could not diſcern the large veſſels in their tail fins, but when the blood began to coagulate, ſome of the veſſels, in a ſhort time, appeared fifty times larger than their natural ſize. There were other veſſels in which the blood had circulated, and were not ſwelled, but here the blood was at a ſtand, and the veſſels themſelves, by reaſon of their minuteneſs and there being no circulation in them, were not eaſily diſcerned.

Moreover, it was my opinion that many of the large circulations of the blood, which I ſaw, when it began to ſtagnate, were not performed within the coats of the blood-veſſels, but that, when the blood in the arteries was impeded in its courſe, the continued and ſtrong propulſion from the heart cauſed it to form new canals, where the fiſh's ſkin made the leaſt reſiſtance, and that it was by this means the oblique current of blood, ſhewn in *fig.* 14, at E B, was formed. And, indeed, I am of opinion that all thoſe very minute currents, whether we call them arteries or veins, are not performed within the coats of veſſels, but that they are formed where the blood, in its protruſion from the heart, meets the leaſt reſiſtance. But if we ſuppoſe one of the very ſmalleſt blood-veſſels to be provided with a coat, and ſuch coat to conſiſt of three diſtinct membranes, as it is ſaid the coats of the veins and arteries are formed, it neceſſarily follows that the coats of thoſe minuteſt veſſels muſt be of a thinneſs which is inconceivable. For, let us ſuppoſe, as I have often ſaid, that the axis of an hundred globules of the blood, from whence its redneſs proceeds, are no more than equal to the axis or diameter of a large grain of ſand; it follows, that ten thouſand globules of blood may paſs together through a veſſel, no larger than to admit a large grain of ſand to paſs through it. Now, ſuppoſing the coat of ſuch

a veſſel, as will admit a grain of ſand to paſs through it, is twenty-five times thinner than fine poſt paper, how much thinner muſt be the coat of a vein which will only admit the ten thouſandth part of a grain of ſand to paſs through it? Therefore, if theſe very ſmalleſt blood-veſſels are provided with coats, ſuch coats muſt be ſo thin, open, or ſpongy, that the very thinneſt watery parts of the blood can as eaſily paſs through them as water through a fine ſieve.

After this, I took one of thoſe fiſh called a Jack, which I placed ſo that about the breadth of a finger of its tail was above the water, and, upon examining the extremities of this tail fin by the microſcope, I could not perceive the leaſt motion in the blood in that part; whence I judged that thoſe parts of the tail were mortified, for the fiſh, from its appearance, ſeemed to me to have been caught ſome days. But when I examined the tail fin, nearer to the body, I ſaw the blood ſlowly moving in an artery, and, a little nearer to the body, there iſſued from this artery a ſmall branch, through which the blood was carried with a very ſwift current, and this branch, taking a ſmall bending, carried the blood into a vein which brought it back towards the heart. Let G H I, in *fig.* 15, repreſent this artery, in which the blood from G to H was driven forward with its uſual ſwiftneſs, and from H to I its motion was very ſlow; H K M, is a ſmaller veſſel, through which the blood was carried from this artery into the vein L M N, at the point N, and in that vein conveyed back to the heart.

After this, I bought a parcel of Trout, which had been caught in the river Maes two or three days before, and among the reſt was one, not quite ſeven inches long, which, upon view of its ſcales, I judged to be be about five years old. This Trout, whoſe tail fin was a little injured at the extremity, I placed with its tail about a finger's breadth above the water; and, when it was become quiet, I viewed the tail by the microſcope, and I could not diſcover any circulation at the extremity, but, examining it a little nearer to the body, I ſaw the blood in the arteries and veins ſtagnating and coagulated; and I

alſo perceived the arteries to be uncommonly large, which I conceived to proceed only from this, that the circulation being impeded, they were by the impulſe of the blood diſtended to that ſize.

Upon viewing the fin ſtill nearer to the tail I ſaw the blood running both in the arteries and veins, and what ſeemed to me very worthy of note was this: I ſaw the blood ſtagnating in an artery, ſhewn in *fig*. 16, at O P Q, from P to Q, and ſo on towards the extremity of the tail, and in the ſame artery from P to O, and alſo towards the fiſh's body, the blood was carried in its uſual motion, namely, from O towards P. From this artery there aroſe a branch, repreſented at P, in which the arterial blood, after it had been driven thus far, was carried back towards the heart, ſo that this branch, P V, ariſing out of the artery, O P Q, muſt be called a vein.

At about three hairs breadth from the before mentioned artery, there was another artery, nearly of the ſame ſize, which is ſhewn in the figure, at R S T. In this the blood between S and T, and alſo beyond it, was coagulated, and the blood which, from R to S, was driven forward, iſſued into a branch ariſing out of that artery, at S, which alſo led back towards the heart, as is ſhewn in S V; where, at V, both the veins, P V and S V, are joined, and conſtitute a larger vein, V W.

Moreover, I took a Carp, in order to view the circulation, but it was ſo reſtleſs that, after many trials, I determined to wrap it in a cloth, in order to view the fin on its back, which, being nearer to the heart, I judged the circulation might be ſtronger: and here I ſaw the blood moving in an artery, which divided itſelf into two other arteries, ſo ſmall as only to admit a ſingle globule or particle of blood at a time; one of theſe minute arteries was joined to a vein, ſo large that the blood in it began to aſſume a red colour, the other I could not follow with my eye, becauſe its courſe was hid behind a ſmall bone in the fin. *Fig*. 17, A B, is this ſmall artery, which, at B, was divided into two branches, one of which, B C, was joined to the vein D C E, at C; ſo that the blood which, in the artery, was

driven from A to B, from the heart, and took its courfe from B to C, in C, being emptied into a vein, it returned back, from C to D, towards the heart: the other fmall artery, B F, took a courfe out of my fight at F. This circulation gradually decreafed till, at length, I could not perceive any motion in the veffels A B C and B F.

Thefe obfervations I alfo purfued in feveral other forts of fifh, and in all of them the circulation was very apparent, particularly the Flounder, in the tail fin of which I faw fuch multitudes of minute blood-veffels, that I may fay there is not a fpace in that fin, fo fmall as a grain of fand, which does not contain a veffel, either conveying the blood from the heart, or bringing it back towards the heart. This fifh, called the Flounder, is one of the beft adapted to view the circulation of the blood, becaufe it lies quiet a long time, and alfo will live a long time out of the water.

With regard to the inftruments I have defcribed, they might, I doubt not, be made more perfect and convenient; for I myfelf was never inftructed in the working in brafs and iron, otherwife than by obfervation of workmen in the making and ufing their tools; by which means, however, I have been enabled (rather in a rude way) to make thefe inftruments fufficient to anfwer all my purpofes.

Addition, by the Translator.

AS the reader may have a defire to know of what nature were the Microfcopes by which Mr. Leeuwenhoek made the wonderful difcoveries, which are the fubject of his works, the Tranflator has thought proper to introduce, in this place, the beft account of them he has been able to collect.

Mr. Henry Baker, in his Introduction to the ufe of the Microfcope, has thefe words:*

" The famous Microfcopes of Mr. Leeuwenhoek are the moft fimple pof-
" fible, being only a fingle lens fet between two plates of filver, perforated
" with a fmall hole, with a moveable pin before it to place the object on, and

* Microfcope made Eafy, p. 7. and 8. Ed. 1744.

" adjuſt it to the eye of the beholder. Several writers repreſent the glaſſes " Mr. Leeuwenhoek made uſe of in his Microſcopes to be little globules, " or ſpheres of glaſs; which miſtake moſt probably ariſes from their under- " taking to deſcribe what they had never ſeen; for, at the time I am writing " this, the cabinet of Microſcopes left by that famous man, at his death, to " the *Royal Society* as a Legacy is ſtanding upon my table; and I can aſſure " the world that every one of the twenty-ſix Microſcopes, contained therein, " is a double convex lens, and not a ſphere or globule."

And in another treatiſe, publiſhed by Mr. Baker ſome years afterwards, he writes as follows:*

" An accurate deſcription of the twenty-ſix Microſcopes, and objects be- " longing to them, contained in a ſmall cabinet, which Mr. Leeuwenhoek, " at his deceaſe, bequeathed to the Royal Society, was preſented many years " ago to that Society by Martin Folkes, Eſq. and may be ſeen No. 380 of " the Philoſophical Tranſactions. And a farther account, ſetting forth the " magnifying powers and other particulars concerning the ſaid Microſcopes " (which were three months under my examination for that purpoſe), was " preſented by me to the Royal Society in the year 1740, and publiſhed in " Phil. Tranſ. No. 458. But neither of theſe accounts has any drawing of the " Microſcopes: it is therefore hoped the curious will be pleaſed to ſee a draw- " ing of them, taken, with great exactneſs, from thoſe in the Repoſitory of " the Royal Society, which are all alike in form, and differ very little in ſize " from this drawing, or from one another.

" The two ſides of one of theſe Microſcopes are ſhewn at *fig.* 18 and 19: " the eye muſt be applied to the ſide *fig.* 18. The flat part, A, is compoſed " of two thin ſilver plates, faſtened together by little rivets *b*, *b*, *b*, *b*, *b*, *b*. " Between theſe plates a very ſmall double convex glaſs (called by mathema- " ticians a lens) is let into a ſocket, and a hole drilled in each plate, for the " eye to look through, at *c*. A limb of ſilver, *d*, is faſtened to the plates " on this ſide by a ſcrew, *e*, which goes through them both. Another " part of this limb, joined to it at right angles, paſſes under the plates, and " comes out on the other ſide (ſee *fig.* 19) at f; through this runs, directly " upwards, a long fine-threaded ſcrew, g, which turns in, and raiſes or lowers " the ſtage, *h*, whereon a coarſe rugged pin, i, for the object to be faſtened to, " is turned about by a little handle, k, and this ſtage, with the pin upon it,

* Employment for the Microſcope, p. 431. Ed. 1764.

" is removed farther from the magnifying lens, or admitted nearer to it, by " a little fcrew, l, that, paffing through the ftage horizontally, and bearing " againft the back of the inftrument, thrufts it farther off when there is oc- " cafion. The end of the long fcrew, g, comes out through the ftage, at m, " where it turns round, but acts not there as a fcrew, having no threads that " reach fo high. Thefe Microfcopes are plain and fimple in their contrivance; " all the parts are filver, fafhioned by M. Leeuwenhoek's own hand, and the " glaffes, which are excellent, were all ground and fet by himfelf.

" The magnifying powers of thefe glaffes come fhort of fome now made, " but are fully fufficient for moft purpofes. Of the twenty-fix Microfcopes " I examined, one magnifies the diameter of an object 160, one 133, one " 114, three 100, three 89, eight 80, two 72, three 66, two 57, one 53, " and one 40 times."

On Frogs, and the manner in which their young are produced from Tadpoles, with the circulation of the blood ſeen in them: alſo on the ſhape of the component particles of the blood.

WE have two ſorts of Frogs in this country, one which uſed to be found in great numbers about the town of Delft, but of late years only few of them have been ſeen, by reaſon, as I believe, that their ſpawn has been devoured by a ſmall kind of fiſh; and I have ſometimes ſeen this ſpawn in the ſmall ditches which divide our fields, lying in ſuch quantities, that the ſurface of the water ſeemed almoſt covered with it. The other ſpecies of Frogs is much fewer in number, but of a larger ſize, and they leap farther than the others, and the hind parts of their bodies, or rather the thickeſt parts of their legs, are uſed by the French, as good food. Reſpecting the propagation of theſe laſt, as a particular ſpecies, I was for ſome time in doubt, not having obſerved them couple together, nor ſeen any of their eggs or ſpawn.

But one day, at the end of the month of May, walking in a meadow for my recreation, and not thinking of this ſubject, becauſe the time of the other ſort of Frogs laying their eggs was long paſt, my attention was excited by the noiſe or croaking, which theſe laſt mentioned Frogs make both by day and night, in hot weather, whereupon I went to the ſide of the water, and I thought that I ſaw ſome of their eggs adhering to a green leaf in the water, and ſo in fact it proved. Theſe eggs are not ſo eaſily to be diſcerned, as thoſe of the common Frogs, becauſe the glutinous matter or jelly which contains them, ſwims deeper in the water, and is alſo leſs in quantity.

I caufed fome of the leaves, to which thefe eggs or fpawn adhered, to be brought to my houfe, and put them into two earthen veffels filled with the water that runs through our canals; and then I examined thofe eggs by the microfcope, and found that, for the moft part, they were dark coloured on one fide, and yellow on the other: but looking at them again the next morning, I found that the yellow colour was almoft vanifhed, only a fmall part of each egg remaining of that tinge, whence I concluded that the eggs had been but lately laid by the Frog when I took them up.

Moreover, I took feveral eggs out of the tranfparent glutinous matter or jelly, which contained them, and I found this glutinous fubftance, by which the eggs feemed to be preferved diftinct in their round fhape, to be very ftrong and tough, fo that it could not be feparated from the egg itfelf, without being torn; and when I handled it ever fo gently, the egg did not retain its round figure, but broke to pieces. I examined many of the eggs after I had taken them out of the jelly which inclofed them, and found them to be contained in a very thin membrane, formed with black parts or fpots, in fhape agreeing with the knobs or protuberances in that leather which is called fhagreen.

The egg itfelf, as far as I could difcern, confifted of a watery matter, (as it appeared to the fight) and an incredible number of globules, each of which again confifted of a great number of leffer globules, all having a larger globule in the middle; fo that every one of them had the appearance of an egg with a very fmall yolk.

The figure of many of thefe eggs altered every day; for, from being round, they affumed an oblong fhape; afterwards minute tails were formed; and I alfo thought that I faw the appearance of heads.

Every day I opened feveral of thefe eggs, and until the feventh day after they had been brought to my houfe, when I faw the young Frogs (which are called Tadpoles) to be fo far formed, that they began to move. But, with all my attention, I could perceive

nothing in them except globules; and upon opening a Tadpole, which of itſelf crept out of the jelly which had contained it, and was ſwimming in the water, and in which, when it was entire, I could diſtinguiſh the back bone, I could not, when diſſected, diſcover any bowels, much leſs arteries or muſcles.

Hereupon it ſeemed to me, that the whole body of this animal, called a Tadpole, conſiſted of no other particles than globules, and eſpecially the belly of it, which was of a yellowiſh colour, and ſeemed to be made of that part of the egg which had continued yellow. This ſeemed ſtrange to me, that in ſo large an animal which I had killed and diſſected, I could not diſcover any veſſels or fibres.

After all my obſervations reſpecting theſe eggs, I could draw no other concluſion, than that the glutinous matter or jelly, encompaſſing the eggs, was only formed by nature for the preſervation of the egg when depoſited in it, as the ſhells of birds' eggs cover and protect the whites and yolks.

And as we ſee, that the whole ſubſtance of a hen's egg paſſes into and becomes part of the chick, (except the ſhell and the membrane within it, which are deſigned for the preſervation of that ſubſtance), ſo in the preſent caſe, the whole of the egg paſſes into the body of the Frog or Tadpole, and the viſcous and gluey matter, which ſurrounded the egg, remains after the young has left it. So that we may ſay of the eggs of Frogs, what I have ſaid of the eggs of birds, namely, that they are deſigned to ſupport and nouriſh the young creature until it is able to provide for itſelf.

When I obſerved that the viſcous matter or jelly I have mentioned, contained in it multitudes of air bubbles, I concluded the uſe of them was, that where there were no leaves floating on the ſurface of the water to which the eggs could adhere, theſe air bubbles might make the jelly float on the water, whereby the eggs might receive the warmth of the ſun, and thereby, as I may ſay, be hatched.

Plate XVII. *fig*. 20, reprefents the egg of the Frog I am now defcribing, inclofed in the tenacious or vifcid fubftance I have mentioned; and when it is fo far grown, that the animal begins to move, the tail appears fomewhat bent. *Fig*. 21, reprefents the animal, when fo fully formed in the egg, as to be able to fwim in the water; this was taken by me out of the egg, and laid on a glafs, where it died, and the body of it dried up.

Fig. 22, A B C D E F, reprefents the fame animal as it appeared to the limner through the microfcope, in which the head can eafily be diftinguifhed from the fore part of the body, as is reprefented at A B F.

F E is the belly of the animal, which was of a yellowifh colour, being formed, as I have faid, out of that part of the egg, where there remained a yellow fpot; but this drawing is not exactly like the animal, becaufe it was fo broken and torn, that it only exhibited the appearance of globules.

C D E reprefents the tail of the animal, in which a fpine or back bone could very plainly be diftinguifhed, and is here reprefented in the figure in the beft manner the limner was able; and though I opened many of the tails of thofe animals, which had this appearance of a fpine, I could not difcover in them any other parts than globules.

Thefe little animals, or Tadpoles, in fwimming, move their tails with great force, and as foon as they ceafe the motion, they fink to the bottom of the water, whence it appears that their bodies are fpecifically heavier than water. But thefe animals have a property (while in their Tadpole ftate) to fix their heads to the glafs, fo that they are able to faften themfelves to any thing they find in the waters, and there to remain without finking to the bottom.

Moreover, I placed a Tadpole, which was alive in the water and had fixed itfelf to the glafs, before a microfcope, directing the limner to make a drawing of it as it appeared to him.

Fig. 23, G H I K L M N O P Q R S, reprefents this Tadpole alive,

as it had fixed itſelf to the glaſs, with its belly next the eye of the ſpectator; this Tadpole had but a few hours before quitted the egg and jelly in which it had been incloſed.

L M N O P, repreſents the head; H I R S, the belly; and G H S, the tail. In the upper part of the head of this animal, its ſkin appeared ſomewhat thicker than the reſt; ſo that I concluded, this was a part of the ſkin which in time would cover the whole body of the animal: this is repreſented at M N O. The mouth is ſhewn at T, which I did not perceive this newly hatched animal to move. At V V are two dark ſpots in the animal's head, of a very round ſhape, and being much more protuberant than I have ſeen in any other animal, and which many would conclude to be the eyes. But if eyes, they would not be ſeen by us in this poſition of the animal; but rather be placed on the other ſide out of ſight.

I K L, and P Q R, are ſix tranſparent parts hanging down from the head, three on each ſide, and theſe parts were the only reaſon of my giving a drawing of this Tadpole; for in each of them I, with great delight, ſaw moſt diſtinctly the circulation of the blood, which was driven from the parts next the body to the extremities, and there, with great ſwiftneſs, performed an inceſſant circuit back to the body. This circulation had not an equable or even motion, but was performed at intervals by quick repeated protruſions; and in the intervals, any one for the moment, would have thought the circulation ceaſed; but ſcarcely had the current of the blood begun to make a ſtop, when a new protruſion immediately followed, cauſing a continual courſe of the blood in this creature; and, upon accurately computing the time in which their pulſations or protruſions were performed, I may ſay, that an hundred of them were performed in leſs time than one could count an hundred. From theſe appearances, I concluded that at every one of thoſe protruſions, the blood was driven out of the heart, and I could perceive this motion (namely, the protruſion of the blood from the heart, and its paſſage from the

arteries into the veins, where they are united), as diftinctly as any perfon can poffibly imagine.

When thefe Tadpoles were fome days old, thofe dependent parts, wherein the circulation of blood I have defcribed was performed, were no longer to be feen, being, as it feemed to me, grown over by the animal's fkin. But I even then faw fomething of a rapid motion, as before defcribed, performed on each fide the head, though I could not diftinguifh it to be the circulation of the blood; at the fame time, the head became fo conjoined to the body, that it could not be diftinguifhed from it.

The circulation of the blood, as seen in the tail of the Tadpole, has been noticed in another place,* therefore no more need be added here, than that my obfervations on this creature were not made upon a fingle infpection of the fubject; but I often repeated the fame, on account of the pleafure I took in the fight, and in different Tadpoles, but I always found the appearance to be the fame. And one thing is here worthy of note, that in the very fmall veffels at the greateft diftance from the heart, as in the extremity of the tail, there did not appear fuch a forcible and vehement protrufion as in the veffels near the heart; but though the blood in thofe fmall veffels appeared to move in an uninterrupted courfe, yet it could plainly be feen, that at every pulfation of the heart the courfe was a little accelerated.

I have in another place faid, that the particles or globules of blood, from whence its rednefs proceeds, are fo minute that a million of them taken together would not equal the fize of a large grain of fand, and from hence we may eafily conclude how exceffively fmall muft be the veffels in which this circulation is performed.

As many perfons have never feen a Tadpole, I have caufed a drawing to be made of this creature, which, defpifed as it is, has afforded me the greateft pleafure; for the circulation of the blood, in this animal, is more admirable to behold than in any other creature or fifh I ever faw: and fuch has been my delight in viewing it, that

* Vol. I. p. 92.

I think no fountain whatſoever, either natural or artificial, or indeed any other ſpectacle, could have afforded me ſuch pleaſure as theſe ſmall animals.

Fig. 24, repreſents a Tadpole, when grown to ſuch a ſize that both its hind feet were formed, and it could uſe them; the two fore feet were alſo viſible, but as yet lay hid under the ſkin.

Moreover, I examined the Tadpoles when they were ſo far perfected as to become Frogs and leap about the fields, and in theſe I alſo diſcovered a great number of wonderfully minute blood-veſſels, which with their bendings conſtituted thoſe veſſels we call arteries and veins; ſo that it moſt manifeſtly appeared to me that the arteries and veins are only a prolongation of one and the ſame veſſel. And this was moſt particularly manifeſt in the extremity of thoſe parts in the foot, which may be called the fingers, and of which the Frog has four in each of the fore feet and five in the hind feet.

Theſe blood veſſels, which we call veins and arteries (though in fact they are one and the ſame), were to be ſeen in great numbers at the extremities of thoſe fingers, and each of them had a round bending, ſo that the diſtinct courſe of each of them could not individually be diſcovered. All theſe veſſels were ſo ſmall and thin that they only admitted the paſſage of the blood in ſingle globules. But when I examined them at the firſt or ſecond joint of the fingers, then I ſaw the arteries and veins were larger, ſo that the blood in them was of a red colour.

I did not only examine theſe young Frogs in parts of their bodies only, but placed their whole bodies before the microſcope, and the before mentioned blood-veſſels appeared to me as I have already deſcribed them. This circulation I ſhewed to two reſpectable gentlemen, who could not behold the ſight without great admiration, eſpecially in thoſe places where they ſaw the blood running in ſuch ſmall veſſels as to be only pervious to one of thoſe particles, whence its redneſs proceeds.

I alſo examined larger Frogs, and in their feet I ſaw the circulation of the blood, but with much difficulty, and, unleſs I had before diſcovered it in the ſmall ones, it would have been impoſſible for me to diſcover the complete circulation in the ſmallest veſſels. But when I examined other parts of the bodies of theſe larger Frogs, I there completely diſcovered the circulation.

Among other things, I at one time ſaw the blood in an artery (large enough to admit three globules of blood at a time) driven in a contrary direction to the ordinary one; but this retrograde motion laſted no longer than while one could tell four, after which it reſumed its uſual courſe. To illuſtrate this: I ſaw the blood running as deſcribed in *fig.* 25, N R O P, paſſing from N to O; out of which artery aroſe a branch or ſmall artery; but here the ſight which attracted my notice was, that the blood in the artery, P Q, not only ſuddenly ſtopped in its motion, but was driven back from Q to P, and emptied into the artery N R O P. The cauſe of this, I think, might be, that the blood in the ſmall artery, P Q, or in the ſmaller branches into which the artery, P Q, was divided, had met with ſome ſmall obſtruction, or that ſome muſcle, adjoining to theſe ſmall veſſels, ſo preſſed upon them, that the courſe of the blood was thereby impeded, whence the blood was not only ſtopped in its progress, but driven by a backward motion into the artery adjoining; for, after the ſhort time I have mentioned, the blood reſumed its motion in the ſame courſe and with the ſame ſwiftneſs as before.

In another place I ſaw the courſe of the blood, in the ſame kind of artery, very much retarded for a ſhort ſpace, and immediately afterwards in the ſame artery a ſudden protruſion, and directly afterwards another ſtop or ſhort interruption in the circulation. This protruſion and interruption took place five or ſix times ſucceſſively, after which followed a ſwift and regular motion, and all this was performed in ſo ſhort a ſpace of time, that I could ſcarcely have ſpoken ten words in the interval.

In my ſeveral obſervations on the circulation of the blood in fiſhes I have not been able clearly to ſatisfy myſelf with regard to the ſhape of the globules or component particles of the blood, for they ſometimes appeared of a ſpherical, and ſometimes of an oval and even a flat ſhape, and ſometimes an irregular figure; this I ſometimes attributed to my glaſſes not being of ſufficient magnifying power to diſtinguiſh them, and ſometimes to the poſition in which they appeared to the eye, for, while in circulation, they tumbled one over another, ſometimes preſenting one part and ſometimes another to the view; and I alſo thought that it might be owing to the ſtraitneſs of the veſſels, in which the particles of blood, being of a yielding nature, might, by the compreſſion, loſe their ſpherical figure.

In order to ſatisfy myſelf in ſome degree on this head, I cut off pieces from the tails of ſeveral ſmall flat fiſh, ſuch as Plaice and Flounders, in order to view the blood when drawn out of the veſſels, and the rather, becauſe I could not perſuade myſelf, that the natural ſhape of the particles of blood in fiſhes was an oval; foraſmuch, as a ſpherical ſeemed to me to be the more perfect form. For I was of opinion, that the particles of blood in fiſhes were compoſed of ſix globules, in like manner with the blood in man, and in terreſtrial animals: and I ſeveral times ſaw the particles of fiſhes blood, the original texture of which was broken, and in which I could diſtinctly ſee four or five, and in ſome few of them ſix component particles. I, however, thought it worthy of note, that many of theſe particles of blood appeared to me of an oval ſhape, ſome few others roundiſh, and others of a perfect ſpherical figure.

In order farther to proſecute my inquiries on this ſubject, I took the blood of a Salmon not quite dead, which was received into a glaſs tube, about the ſize of a ſmall writing pen: this blood, after a ſhort time, became coagulated; but having reſtored it in part to its fluidity, I put it into a ſmaller glaſs tube, in which I viewed it, holding it ſo, that the particles of blood might be kept in motion continually, by which means many of the particles appeared before my

fight with a flat and oval fhape; in others, the fides of which were turned towards me, I could fcarcely perceive any fenfible thicknefs; and in fhort, others, where their fides were not exactly turned towards me, appeared fomewhat broader in proportion to their fize; but I could not difcover one particle of blood of a perfect fpherical fhape.

The blood of a Salmon appears, to the eye, of a blackifh colour, by reafon of the very great number of thofe particles which give the blood its red colour, and which are more in number in this fifh's blood than in others.

Moreover, I fpread a fmall portion of this blood upon a clean thin glafs, and I obferved where the particles of the blood lay thinneft, that they were of oval fhapes, and in many of thofe oval particles, it could be feen that they were compofed of globules; and I faw a few of them, wherein I could diftinguifh fix globules lying in two rows, three and three.

But where the particles of blood lay in numbers clofe together, they were fo coagulated, that no oval figures could be feen in them, and all I could obferve was fome confufed particles, fix of which made up one entire particle of blood.

Thefe renewed inquiries of mine were, with intent to difcover whether I had before been in an error, by faying, that the particles of blood in fifhes, were not oval, but fpherical; becaufe all thofe perfons who had feen the circulation of the blood at my houfe, were of opinion, that the particles were not oval, but fpherical.

I have heretofore faid, and do ftill believe, that the blood-veffels in many parts where I have viewed the circulation, are fo extremely flender, that if a large grain of fand was divided into a million of parts, not one of thofe parts could pafs through thofe fmall blood-veffels, unlefs they were as foft and flexible as the particles of blood.

I have ufed all the diligence in my power, in order, if poffible,

to difcover thefe oval particles at the time the blood was in its natural courfe in the veins; for which purpofe, I felected for examination, the very fmalleft blood-veffels; but though I was very attentive, I could not fatisfy myfelf, for fometimes I faw a darkifh particle, then one much more tranfparent; and when I viewed the very fmalleft veffels, in which the blood had little or no motion, as divers veffels at the extremity of the fins, the particles of the blood lay fo very thin and fcattered, that I could not fee any thing except a liquid without a motion, which was fomewhat of a yellow colour.

I have heretofore caufed drawings to be made of the particles of blood, reprefenting them of an * oval fhape, though the limner had not the originals before him. I have alfo diluted blood with water, becaufe the multitude of its particles impeded a just view of them; but now, for the greater fatisfaction of myfelf and others, I gave into the hands of the limner a microfcope, before which was placed a portion of the blood of a Salmon, in order that he might make a drawing of the particles as they appeared to him.

Fig. 26, A B C D, fhews the oval particles of the blood of a Salmon, which weighed about thirty pounds.

At A B are reprefented thofe particles which did not come in a ftraight line before my eye; the others, fhewn at C, floated ftraight before the fight: in moft of them was a luminous fpot, though in fome larger than in others; thefe the limner alfo obferved, and reprefented them as nearly as he could in the middle of the particles in the drawing. Thefe particles appeared black, and I difpofed the ferum in which they floated, in fuch a manner as to make them fink to the bottom, though with fome difficulty; and in this pofition the limner had an opportunity of feeing fome thoufands of thofe particles.

* See Vol I. p. 94.

If I had made a drawing of theſe particles, as they appeared to me, I ſhould have repreſented them twice the ſize here ſhewn, ſo that here is an inſtance of the diverſity of ſight in different people.

Moreover, I placed before a microſcope the blood of a ſmall Flounder, not diluted with any other liquor, but the particles were floating in their own proper ſerum; theſe particles are ſhewn at *fig*. 27, between E and F.

Although theſe particles are repreſented ſomewhat ſmaller than the former ones, I could not perceive any difference in their ſize, and I am certain, that there is not ſuch difference, but that the particles of blood, whence its redneſs proceeds, are no larger in a whale than in the ſmalleſt fiſh.

I alſo put ſome particles of the blood of a ſmall Flounder in a clear and very clean glaſs, and placed them before a microſcope of leſs magnifying powers than the former, in order that I might ſhew them, even in the dark days of winter, by day-light alone. Theſe particles of blood, in the middle of which the light could be ſeen, and which was obſerved by the limner, are ſhewn at *fig*. 28, between G and H.

I had alſo ſome blood lying in a glaſs before deeper magnifiers: from theſe the thin ſerous liquor was evaporated, and in ſome few of theſe oval parts (which were ſo ſeparated, that they did not touch one another), it could be ſeen that each of them, as far as the eye could diſtinguiſh, conſiſted of ſix globules: theſe ſix globules the limner imitated as near as he could, as appears at *fig*. 29, between I and K.

After theſe obſervations, I deviſed means to view the circulation of the blood through deeper magnifiers than what I had uſed before, in which attempt I ſucceeded ſo as to ſee the rapid courſe of theſe oval particles in the blood-veſſels, by ſeveral different microſcopes, and the greater the magnifier, the plainer was the courſe of

blood to be ſeen: in order to abate this rapid motion, I ſometimes preſſed the ſmall arteries for the ſpace of two minutes, when I ſaw in divers ſmall blood-veſſels, ſome oval ſhaped particles were ſo ſeparated from each other, that in ſuch veſſels no particles of blood could be diſtinguiſhed, not even thoſe of which ſix go to make up one entire particle of blood, but only a fluid ſubſtance paſſing through the veſſels, which was almoſt colourleſs, and in one of the large blood-veſſels in the tail, which was an artery, the blood ran very ſlowly: in this laſt veſſel I plainly ſaw that the particles of blood in it were oval, and I not only ſaw them plainly, but I could, more diſtinctly than before, obſerve the component globules, of which all, or the greater part, of theſe oval particles were compoſed.

It is eaſy to conceive how ſix globules of a yielding or flexible nature, in continual motion and ſtriking againſt each other, may form a perfect ſpherical figure, as I have elſewhere obſerved: for example, let *fig.* 30, repreſent the original compoſition of a particle of blood, conſiſting of ſix globules, five of which appear to the eye, and the ſixth is out of ſight. I have myſelf with wax made up ſuch a globule, conſiſting of ſix ſmaller ones, as is pictured at *fig.* 30 and 31, and each of thoſe ſix, of ſix others in order to expoſe to the view of the curious, the make of theſe globules of blood; for I may lay it down as a fact, that each globule of our blood conſiſts at leaſt of thirty-ſix globules; theſe before mentioned globules, preſſed together, in conſtant motion, and flexible in their nature, are, as it were, mutually compacted together, and aſſume a perfect ſpherical ſhape, as ſhewn at *fig.* 31.

From this diſpoſition of the parts, we may conceive how the globules of blood, in men and animals, have a round figure, but how thoſe oval particles of blood I have been treating of are compounded, and made up of ſix globules, is not eaſy to comprehend.

I have before ſaid, that I believed every globule in our blood, I mean thoſe, ſix of which go to make up a perfect globule, as is ſhewn in *fig.* 30, is itſelf compoſed of ſix globules, and who knows how this is performed? For in how ſmall parts ſoever we, in imagination, divide a globule of blood, there may, nevertheleſs, be particles of which ſuch a globule conſiſts, infinitely ſmaller, and I wonder that any will be ſo bold as to publiſh what they do, reſpecting the original particles of matter; for my part, I think, that could I divide, even in imagination, a globule of blood into a thouſand million of ſmaller particles,* I ſhould not go the extent of its component parts.

Since we now ſee, as before is obſerved, that the particles of blood can, by preſſure, be divided, and ſo united with the thin liquor in which they float, that we take it for a ſimple or uncompounded liquid, we may imagine, than when a horſe preſſes his breaſt with a heavy load, the globules of blood in the veſſels, where the preſſure is greateſt, may be diſſolved or divided in the veſſels, and united with the fluid, which phyſicians call the ſerum.

I have alſo thought, whether or no the particles of blood, ſo divided or diſſolved, may not, when removed from the incumbent preſſure, aſſume their priſtine figure, in like manner with the particles of fat or tallow; for, if a portion of ox's or ſheep's tallow is expoſed to the fire, the particles of fat, which we call globules, as having the neareſt reſemblance to that ſhape, are diſſolved and exhibit only a clear tranſparent liquid, even to the microſcope, but when the heat ceaſes it again appears in the ſhape of globules, and if melted ten times over it ſtill, when cold, aſſumes the ſhape of ſpherical particles.

I have likewiſe laid it down, as my opinion, that no blood, which is carried in a vein to the heart, can become arterial blood, unleſs

* The Latin tranſlation has it *decem millies*, ten thouſand: but the author's own words are, *duyſent millioenen*, a thouſand millions.

it has firſt paſſed into the heart; but ſince, in one of the obſervations I was making only for my amuſement I ſaw the contrary, I have cauſed a drawing to be made of that appearance, in which drawing I directed the blood-veſſels to be repreſented rather larger than they appeared to me, and in the middle of the ſmall veſſels there are no dots: thoſe dots, in other parts of the figure, denote the globules of blood, in order more plainly to deſcribe the circulation.

Let us ſuppoſe A B, in *fig.* 32, to be a vein in which, by the microſcope, the blood may be ſeen driven with great ſwiftneſs from B to A. From this blood-veſſel iſſued two ſmall branches, repreſented at the letters C and D, which were united between the letters E and F.

On the other hand, the letters H I exhibit an artery through which the blood was, with like ſwiftneſs, driven from H to I, and from this artery, H I, proceeded a branch, ſhewn at the letters K F L.

Now the blood running from K towards F is united to the veſſel F at that letter, and by this means the blood iſſuing out of the artery is in part infuſed into a vein and carried from F to G; and the ſame quantity of blood (as according to my moſt accurate obſervations appeared) as was carried from K F to G downwards, ſo much blood of that, which was carried from C E to F, was carried upwards from F to L; ſo that as much of the arterial blood as the veſſel, K F, conveyed into the veſſel, F G, ſo much of the venal blood did the veſſel, C E, convey into the veſſel, F L. And, though I have often enjoyed great pleaſure in viewing the circulation of the blood, this ſpectacle, which I have been juſt deſcribing, was more delightful than any other, becauſe I could moſt clearly and diſtinctly ſee the objects I have deſcribed, and alſo, becauſe I never before ſaw ſuch a conjunction or communication between the blood-veſſels.

On PHOSPHORUS. *

A CERTAIN German gentleman, who ſaid he was a Doctor of Phyſic, and newly come from England, paying me a viſit, with the compliments of ſeveral of my friends in London, after ſitting with me sometime, produced a ſmall veſſel filled with water, at the bottom of which lay ſome ſmall pieces of a whitiſh ſubſtance, inclining to a tinge of yellow; theſe he took out of the water, and with one of them he traced upon paper ſome letters about the ſize of a joint of one's finger; and though at firſt, nothing of theſe letters was to be ſeen on the paper, yet upon removing it into a dark place, the paper ſeemed to be on fire in every place where the letters had been traced: but this fire was very pale or faint, and ſeveral hours afterwards viewing the paper in the dark, the letters had ſtill a lucid appearance. He alſo took a ſmall piece of this ſubſtance and put it between two pieces of whited brown paper, and rubbing them briſkly ſix or eight times with his cane, in the place where the before mentioned ſubſtance lay, to my great ſurprize, I ſaw the paper, by means of the friction, burſt into a flame.

This medical gentleman told me, that this ſubſtance was prepared by diſtillation from urine which had been long kept, and that it could not be preſerved, unleſs kept under water: he gave me a piece of it, and with a particle thereof, about the ſize of a pin's head, I repeated the before-mentioned experiments three ſeveral

* Mr. Leeuwenhoek calls this ſubſtance *ligt der nature*, which the Latin Tranſlator renders *lumen naturale*, in Engliſh, natural light: from the deſcription here given, it can be no other than the Phoſphorus of the ſhops.

times, not only with the ſame event, but I alſo found, that when the letters had been ſo traced, and the paper was brought ſo near the fire as to warm it, the places where the letters had been traced, were immediately inflamed, and the flame ſpread farther over the paper.

Soon after this, a friend of mine, who was on his travels, ſent me, by a meſſenger, a ſmall veſſel full of water, in which was a ſubſtance about half as big as a pea, and informed me that it was a mineral, and called natural light; being a part of what had been given to a chymiſt in the town where he then was, by a perſon who had received it as a preſent from ſome other, and the half of it was now ſent to me. My friend alſo informed me, that he had himſelf ſeen, that when taken out of the water and laid in a perſon's hand, a vapour and ſmoke iſſued from it, and, upon being carried in the hand into a dark place, it emitted a light ſimilar to that produced from rotten wood. Upon viewing this, I immediately judged it to be the ſame kind of ſubſtance as that which I had received from the German gentleman; for, upon viewing it with the naked eye, and trying the ſame experiment of its effect, the whole were as exactly alike, as if it had been broken off from the ſame piece; I thereupon ſet about a diligent examination of this ſubſtance, and noted the following particulars in it.

I poured the water and this ſubſtance in it out of the veſſel, and cut off, while it was under the water, a piece or particle, about the ſize of a pin's head, having firſt prepared a glaſs to receive it and make my obſervations. Having put this particle on the glaſs, I immediately placed it before the microſcope, and ſaw lying about it a ſmall portion of ſome kind of moiſture, and the particle itſelf appeared like a dark body, excepting only, that in two places, there was a ſmall luminous appearance: I alſo ſaw a great number of particles, which I deemed to be globules, in violent agitation one among another; theſe globules were collected in two places on the

particle, and then were, as it were, driven off from it, and when they had got to about five or fix hairs' breadths from the fubftance of the particle, they difappeared. All thefe globules, viewed by the naked eye, exhibited the appearance of fmoke: foon afterwards, feveral fmall luminous parts appeared on the particle, which by degrees increafed fo far, that through the microfcope, they gave me the reprefentation of a piece of burning turf, with fome afhy parts here and there covering its furface.

But what I moft wondered at was, that what I have faid gave the appearance of fmoke, did not difperfe itfelf in the air, nor mount upwards, as we obferve common fmoke to do, but fell back on the glafs, fo that round about this particle of fubftance, called natural light, there was not only more moifture than before, but at the diftance of half an inch from it, there lay a very tranfparent moifture or liquor, confifting of minute round particles of different fizes, and thefe in fuch numbers, that it was wonderful to behold: many of thefe feemed to be watery, others oily particles.

After this particle had laid a whole night, I examined it again in the morning, and then I faw that the fubftance which feemed watery, was increafed, and that the minute globules, exhibiting the appearance of fmoke, ftill iffued from the particle. I then carried it into a dark place, to fee whether the fame kind of light was emitted from it as I had feen the evening before, but I could not diftinguifh any.

After it had remained twenty-four hours in my clofet, expofed to the air, I again examined it, but I could not perceive any thing expelled from it. I then lighted a candle, excluding the day-light, that I might fee more acutely, and then I faw fome globules expelled, which I think was principally caufed by the heat of the candle; for I obferved, that all the moift part of the fubftance which had been expelled, was evaporated, and fome faline particles of irregular figures were coagulated.

Moreover, I prepared a glafs tube, in which no liquor had ever been, and taking one half of this fubftance, called natural light, out of the water, I wrapped it in blotting paper to dry up any part of the water which might adhere to it. Being thus dried, I put it into the glafs tube, with intent to clofe the orifice by heat, firft ufing the neceffary precaution that the glafs might remain cold, excepting the bare end, which I clofed by applying it to the flame of a candle.

Having thus clofed the glafs, I examined the fubftance I had put into it, and faw an incredible number of globules iffuing from it, producing round about it an appearance of vapour or fmoke, and at length forming a collection of watery and oily matter, which gathered about the particle in fuch quantity, that, after an hour's time, they impeded my view of the globules which were expelled: whereupon by ftrongly fhaking the glafs, I removed the particle into a frefh place, and then, again examining it, I faw the globules expelled from it in as great numbers as before, and after three hours, they formed fuch a collection of moifture, as again to intercept my view of the globules as they were expelled.

I brought the glafs, containing this fubftance, near to the heat of the candle, and immediately placed it before the microfcope; and then I faw, not only a great number of globules expelled from it, but alfo many globules expelled from that part which had been converted into a liquid; and in fome places I faw that the heat had changed the liquid into hard or folid corpufcles, to which, by reafon of their minutenefs, I could not affign any determinate fhape, and which, to the naked eye, reprefented a white appearance.

I again brought the glafs to the candle, and expofed it to a greater degree of heat, imagining that when, in this clofed glafs, I had driven the liquid from one place, I fhould find it collected in another, but in this I was miftaken, for all the moifture was changed into rigid corpufcles, and though, on the next and following days, I

examined the fubftance in the glafs, I could not perceive any moifture.

The piece of this fubftance which I had left remaining I put into a glafs tube, the cavity of which was not larger than the tip of a child's little finger, whereas the former tube was more than three times that fize. This I examined by the microfcope, placing the particle in a fomewhat oblique point of view, and then, not without wonder, I perceived the globules expelled from it; which, to the naked eye, appeared as fmoke, not rifing up on all fides, as is univerfally obferved in common fmoke, but, on the contrary, that they tended downwards.

To illuftrate this, let A B C D, Plate XVIII. *fig*, 1, reprefent this piece or particle of fubftance, which is called *natural light*, and let A be fuppofed the lower and C the upper fide of it, and in this pofition the globules, expelled between E D and G, were driven downwards, that is, towards H, and, in like manner, the globules between E B and F were driven towards H; and when I made B the lower part, all the expelled matter took its courfe towards K, and if D was made the lower part, then it all tended towards I. This expulfion of globules lafted only two or three days, and, as I faw no farther alteration for the fpace of two or three weeks, I laid the glaffes afide.

This fubftance was fent to me on the fifteenth of April, 1693, and in the middle of Auguft, in the fame year, I looked at the firft mentioned glafs, when I was greatly furprifed to find that it feemed to be all vanifhed out of the glafs, whence, at firft, I doubted whether this was the real glafs which I had ufed: but, feeing a great quantity of tranfparent moifture in it, I became more fatisfied that it was the same, and the more fo when, upon warming the glafs and the moifture which adhered to it, towards the warmth I faw the moifture immediately change into a white fubftance. I then examined the other, and the fmaller glafs tube, which was still clofed up, and I could not difcern any alteration in the contents.

In order to give a better idea of the manner of making my obfervations on this fubject, I will explain how I prepared the fecond and fmalleft glafs tube which I ufed.

I took a glafs tube, of the length and fize reprefented at *fig.* 2, A B, and by the help of a brafs pipe, which goldfmiths and other artificers ufe to folder fmall pieces of work, which they call a blow-pipe, and by applying with it the flame of a candle I blew at the end of it the fphere or glafs globule, *fig.* 3, F D E.

When this fphere and the whole glafs was cooled, I dropped into the opening, C, the particle of the before mentioned fubftance, called *natural light*, which refted at the place E: I then took the fphere between my fingers, that when I fhould again approach the tube to the fire, there might the lefs heat reach to the fphere, then, directing a ftream of fire to the tube, at G, until the glafs was hot enough to be extended, I gave it the figure fhewn at *fig.* 4, H I K L. I then broke the glafs at the flender part, H, and the tube then was of the fhape fhewn at *fig.* 5, M N O P, and when the glafs was again cool, I brought the orifice, M, to the flame of the blow-pipe, whereby the glafs immediately melted, and the orifice was clofed. This glafs having remained clofed up for fo long a time as I have mentioned, I broke off a fmall piece at M, caufing an aperture about the fize of a pin, and from which the air rufhed out with fo much force as to produce fomething of a noife. I then immediately obferved the fubftance in the glafs, and faw the globules driven from it in as great numbers as if it had been but newly put into the glafs. I examined it feveral times the fame day, and conftantly faw the expelled matter tending downwards, as I have before defcribed.

The following day the matter was fo diminifhed that two fmall cavities were formed in the middle of it, and the moifture was fo much increafed that the matter fwam in it. In the evening of that day two third parts of the folid fubftance were wafted: I then took the piece out of the water, and faw the globules expelled from it as large as ever.

On the morning of the third day after the glaſs had been opened, I examined it, and ſaw that but a ſmall part was remaining, yet the globules were expelled with the ſame force; in the evening, about nine o'clock, it was diminiſhed to leſs than the ſize of a large grain of ſand, and yet globules were expelled from it, though in no great number, by reaſon of its minuteneſs.

On the fourth day there remained no more than the ſize of a ſmall grain of ſand, yet the expulſion of globules ſtill continued: in the evening there was nothing remaining of it deſerving to be mentioned, nor did I ſee any more globules iſſue from it.

During the courſe of theſe my obſervations I have often conſidered what might be the cauſe why flame is ſo eaſily produced from this ſubſtance, but have not been able to ſatisfy myſelf on that head. This, however, ſeems certain to me, that this ſubſtance may, in great part, be kept out of water undiminiſhed, provided it be preſerved from communication with the air, and cloſed up in ſome veſſel, as I have mentioned.

If any perſon ſhould imagine, on reading my deſcription of the glaſſes fabricated by me for theſe and my other obſervations, that I am verſed in the art of glaſs-blowing; I muſt inform him, that the only knowledge I have therein was acquired from thoſe artiſts who, at our fair-times, came to exhibit the manner of blowing in glaſs by the candle or lamp; and, by obſerving their manner of working, I have learned ſufficient to qualify myſelf for preparing glaſſes to anſwer my ſeveral purpoſes.

The Sting of a Gnat, as defcribed by Swammerdam, fhewn to have been erroneoufly pictured by him. The Author's defcription of the Gnat's Sting, and alfo of that of the Horfe-fly, and the Feathers on the wings of Gnats.

DOCTOR John Swammerdam has lately printed and publifhed to the world a figure of the Gnat, as drawn from the microfcope, and particularly its fting out of the cafe or fheath, which fting is pictured exceedingly pointed and flender, and extended to a great length from the fheath. Upon viewing this figure, I could not perfuade myfelf that the fting was, in reality, formed in the manner there exhibited; for I was well affured that, if of fuch a fhape, it muft either be bent or broken when thruft into the fkin; nor could I eafily believe that in the head of a Gnat, or in the fore part of its body, there could be contained mufcles or organs of fufficient length and ftrength to give the fting its necessary firmnefs. For thefe reafons I determined to examine into the true formation of the Gnat's fting, and I was alfo defirous of difcovering, if poffible, the reafon of the great pain it excites, and of the fwelling on the fkin caufed by the puncture.

I could not difcover that the Gnat, when ftinging, protruded the fting from the extremity of the fheath (though I thought I faw, rather obfcurely, a fmall part of the fting fo protruded), but I always found that the Gnat made a wide opening on one fide of the fheath, in like manner as if one was to hold a fword in a fheath, the leather fides of which were laid together in fuch a manner that, when the fword was to be ufed, it would not be neceffary to draw

it wholly out, there being an aperture left on one ſide of the ſheath, which might be opened by a touch.

Having taken out the ſtings of many Gnats, through this opening in the ſide of the ſheath, and having examined ſeveral Gnats, who protruded their ſtings, ſome in part and others to their full extent, through the ſame aperture, I fancied that I ſaw the extremity of the ſting to be pointed like a spear, and to be barbed or jagged on each ſide. But, on more narrowly examining the ſtings I had extracted, I found I had been miſtaken in this: that what I at firſt took for one ſingle ſting was, in reality, compoſed of four parts, for, out of what I before deemed to be only one I took two ſtings, each having hooks or barbs at the ends; the third part, from which I took thoſe two (and which might alſo be called a ſting), was open on one ſide, in like manner as I have mentioned reſpecting the ſheath, and terminating in a point, and appearing, through the microſcope, like a quill cut ſloping: the fourth piece, which was exceedingly thin, ſeemed to be placed round about the laſt mentioned ſting, but, when I examined it more narrowly, I ſaw that it alſo lay in the cavity of it.

When I had ſeparated theſe four pieces of the ſting from each other, they did not preſerve their ſtraight figure, but became ſomewhat bent, eſpecially thoſe two which had hooks on them, ſo that I could not place theſe two laſt before the microſcope to my wiſh. Hereupon I was obliged to cut aſunder thoſe four pieces, and give diſtinct drawings of each of them from the microſcope, in order to ſhew to all men, and in particular to my own countrymen, (who I ſuppoſe are more tormented with theſe inſects than the people of other nations, by reaſon of the many ſtanding waters we have, where they breed in great multitudes), this wonderfully formed and miſchievous weapon in the Gnat.

To place all theſe things in a clear and diſtinct view, I will firſt exhibit the figure of the Gnat's ſting, as deſcribed by Swammerdam, and which he says he himſelf has ſeen. Plate XVIII. *fig.* 6. A B C D E, is, as Swammerdam ſays, the ſting of the Gnat, with its

ſheath or caſe, A B D E being the caſe, and B C D the ſting, which ſting, he ſays, is ſo ſharp at the extremity, that he could not, with his beſt microſcopes, diſcern the point.

Fig. 7, repreſents the ſheath and the ſtings of the Gnat, as placed by me before the microſcope, and drawn by the limner from the life, by my direction. F G H I is the caſe or ſheath, which the Gnat opens on the ſide, at G H I, in order to protrude the ſtings when it is going to ſtrike: this ſheath is covered with hairs, and between them with many ſmall feathers, but which feathers are ſo cloſely joined to the body of it that they are rarely to be ſeen. H K is a part of thoſe four organs or ſtings, as they are regularly placed beſide each other, and at ſuch a diſtance from the ſheath as the Gnat protruded them of its own accord, and not forced out by me, unleſs in killing the animal I did it inadvertently. At K are ſeen the barbs or hooks with which the ſtings are furniſhed. The colour of theſe ſtings is like that of tranſparent tortoiſeſhell. F I is that part of the ſheath and ſtings cut off next the Gnat's head.

Fig. 8, L M N O P, repreſents that part of the ſting, or ſecond caſe, from which I extracted the two other ſtings, the points of which are furniſhed with hooks. This has an opening from end to end, through which the other ſtings may be protruded, in like manner as I have mentioned reſpecting the firſt ſheath: I have alſo, through this opening, oftentimes drawn out all the three ſtings which lie within it.

L M Q O, is another piece, lying upon the former, and, being ſomewhat broader and longer than the other, ſeems as if deſigned to cover it like another ſheath, but I have often drawn it out of the cavity of the piece L M N O P.

Fig 9, R S T, repreſents in part all the ſtings, and here may be diſcerned the two inward ones that are barbed, exhibiting the ſame appearance as at H K. Theſe two interior barbed ſtings are wonderfully thin but not flat, for if ſo they would, by their thinneſs, be unable to bear any force, and ſtill leſs have power to perforate the

ſkin. But they have a third ſide on the back of each of them, and their figure, in that reſpect, is like that of a ſmall ſword, which, by reaſon of its ſmallneſs, muſt neceſſarily have a third ſide to give it ſtrength, agreeing, in that reſpect, with theſe ſtings of the Gnat.

Hitherto it has appeared no otherwiſe to me than that each of theſe barbed ſtings has the barbs or hooks only on one ſide, and when they lie on the inſide of their caſe, or more properly, within the thickeſt of the ſtings, their flat ſides are cloſe together, and thoſe ſides which are barbed lie on the outſide, ſo that when all the pieces of the ſting are placed together in order, they exhibit, when taken out of the ſheath, the appearance of a ſingle ſting barbed or hooked on each ſide.

Fig. 10, V W X, repreſents one of the two barbed ſtings taken out of the cavity of the other piece, one of them being a little longer than the other.

Fig. 11, a b c, repreſents a part of one of the two laſt mentioned ſtings, which, by reaſon of its exceeding thinneſs, was bent in this ſhape, but in this poſition, the hooks or barbs cannot be diſtinguiſhed; placing the flat ſide of this before the microſcope, the point of it appeared as at *fig.* 12. Upon turning it round a little, the point appeared as at *fig.* 13, and turning it ſtill more, the hooks became viſible, as is ſhewn at *fig.* 14.

If we conſider the formation of theſe ſtings, though we know not how the Gnat ſtrikes them into our bodies, or moves them about when there, we may, nevertheleſs, eaſily conceive, that when driven within the ſkin, they may make a very ſenſible, though minute wound, and by reaſon of their length, a much deeper one than is occaſioned by lice, fleas, or other ſmall vermin. Again, ſo long as the Gnat is ſucking the blood which iſſues out of the wounded veſſels, there will not any ſwelling appear. But when it draws out the ſtings, the juices of the wounded veſſels continuing to iſſue, there muſt neceſſarily ariſe a greater ſwelling than uſual, becauſe, as I before ſaid, the ſtings enter ſo deep, and another rea-

ſon is to be attributed to the minuteneſs of the ſtings, which make ſo ſmall a wound, that the ſkin, eſpecially that part next the ſurface of the body, cloſes immediately upon the extraction of the ſting.

If any perſon ſhould be deſirous to follow my example in the examination of the Gnat's ſting, I caution him to arm himſelf with patience in the purſuit. I have often opened the ſheath or caſe incloſing theſe ſtings, and taken them out as they lay regularly placed beſide each other, but to ſeparate the four parts or pieces of this ſting, and to place them before the microſcope, ſo as to give a diſtinct view of them to others, requires no ſmall labour and pains. I have deſtroyed above an hundred Gnats in accompliſhing this purpoſe, and have been obliged to repeat my obſervations many times, for though I could ſee all the pieces, and did my beſt to fix them before different microſcopes, it often happened, that while I was buſied with one of them, I loſt ſight of another; for which reaſon, I was obliged to make new trials and obſervations many days together.

I thought it would not be amiſs in this place to give a figure of one of the two ſtings which the large Flies, commonly called Horſe-flies, carry in a ſheath in their heads near the mouth; which, as drawn from the microſcope is ſhewn at *fig.* 15, A B C D, and I have cauſed this drawing to be made, not only to ſhew the nature of this ſting, being of a flat ſhape and very ſharp, and with which the animal torments horſes to that degree, that they kick and leap about the field even at the ſight of that Fly, but alſo to point out that as the ſting from D to C, is exceeding thin and ſharp; ſo from C to B, where it tapers to a point, it grows thicker and thicker to the very extremity at B, by which means it is of the ſame ſtrength throughout; ſo that, in a word, we cannot but obſerve the greateſt perfection in the formation of the ſmalleſt animals.

The feet of a Gnat, and its whole body, are covered with very beautiful feathers, one of which, drawn from the microſcope, is

ſhewn at *fig.* 17; the wings alſo are covered with feathers. *Fig.* 18, is one of the wings of the ſize it appeared to the naked eye; *fig.* 19, A B C, is the ſame wing ſomewhat magnified, in order to ſhew, not only that the whole border of it, A B C, is covered with large and ſmall feathers, but alſo the nerves or bony parts, D D D D, which give ſtrength to the wings. One of theſe feathers magnified, is ſhewn at *fig.* 20. The membrane or thin ſkin between theſe parts, appears, when viewed by the microſcope, to be covered with a great number of exceedingly minute particles elevated above the ſkin, and theſe, upon a cloſer examination, I found to be, in truth, hairs: they are to be ſeen at *fig.* 21, which is a ſketch of a ſmall portion of the whole wing. A B C, are the feathers on the border, and A D E C, the hairs on the membrane of the wing.

On the Nature of Infenfible Perspiration, with the Author's method of computing the quantity of moifture which iffues from the human body, by that evacuation.

HAVING had a difcourfe with a certain medical gentleman on the fubject of what is called Infenfible Perfpiration, or the great quantity of matter or fubftance which iffues from our bodies, and which we are unconfcious of, I determined to make an experiment on this fubject, by an obfervation of the perfpirable matter iffuing not from my whole body, but from one of my hands only.

For this purpofe, I took a glafs jar, wide enough to admit my hand, which jar, as far as I knew, never had any thing put into it, except clean rain water; and having wiped it as dry and clean as I could, I put my left hand into it, ftopping the aperture round my wrift with a cloth, that none of the perfpirable matter might efcape from the glafs, and I then began to drink tea until it not only warmed me, but brought on a moderate perfpiration.

After fome time had elapfed, I perceived the perfpirable matter iffuing from my hand, collected on the infide of the glafs, exhibiting the fame appearance as when in fummer time, a bottle of wine is brought out of a cool cellar into the warm air, whereupon the moifture in the air will condenfe and fettle on the glafs round the wine. Soon after this, the moifture was fo increafed, that it adhered to the glafs in fmall drops, and at length thofe drops ran down and fettled at the bottom of the glafs. After I had kept my hand in this fituation three quarters of an hour, I took it out of the glafs, and with all the accuracy I was able, I weighed the perfpirable matter which had iffued from it, and found it to be the fixteenth part of an ounce.

In the latter end of the month of January, I repeated my obfervation, by again putting my hand, while it was very cold, into the glafs, and fitting down by the fire, I began to drink tea, fo hot and fo plentifully, as to produce a copious perfpiration; and after keep-

ing my hand in the glaſs a whole hour, I collected the perſpirable matter, and found it to weigh three thirty-ſecond parts, being a ſixteenth and the half of a ſixteenth part of an ounce.

Hereupon, I began to conſider and reaſon thus with myſelf; if the perſpiration in every part of one's body is in the ſame proportion, as in the experiments with the hand, how great a quantity of moiſture muſt iſſue from our whole bodies, and how neceſſary muſt it be when we take any medicine to promote perſpiration, that we ſhould alſo recruit our ſtrength by ſome reſtorative liquor, ſuch as either wine and water ſweetened and boiled with the yolk of an egg, or elſe drink meat broth; eſpecially if we conſider, that the health and ſtrength of our bodies depends on the juices.

In order to make an eſtimate of the proportion the hand bears to the whole body, I filled the before mentioned glaſs jar with water to the brim, and having placed it in a larger veſſel, I thruſt my hand into it: the water which ran over, and was equal to the ſize of my hand, I collected, and found it to weigh eleven ounces. I am indeed aware that we cannot make a true computation of the ſurface of the body by that of the hand, becau e the hand is furniſhed with fingers, from which the perſpirable matter iſſues. However, if the perſpiration is the ſame throughout the body as in the hand, I will venture to ſay, that according to my preceding obſervations, I ſhould perſpire in an hour's time, about the quantity of twenty ounces. For I reckon my body to weigh one hundred and fifty pounds, and my hand eleven ounces, and I compute that eleven ounces weight of water contain eighteen one-third cubic inches, and this I ſet down as the ſolid contents of my hand. Farther, I reckon that ſixty-five pounds weight are a cubic foot of water, and contain 1728 inches; and according to this calculation, we ſhall ſee that the ſize of my whole body is almost two hundred and eighteen times larger than that of my hand.

On the propagation and rapid increafe of the common Fly: the manner in which the common Nettle produces pain and inflammation explained.

A SURGEON of fome eminence in thefe parts, happening to meet with me, fhewed me a piece of glandulous or fungous fubftance, about the fize of a finger's nail, which he had taken from the difeafed leg of a certain gentlewoman, whofe leg from the foot to above the knee, had for fome years been uncommonly covered with thofe kind of tumors, and he told me, that having wafhed this fubftance in brandy, and afterwards cut it open, he had perceived in it a number of minute maggots: thefe he produced to me, but they were fo fmall, that I could not diftinguifh them without my fpectacles. A piece of this fubftance was put into my hands by the furgeon, in order that I might examine into the nature of thofe maggots.

Upon my return home, I examined them by the microfcope, and was immediately convinced that they had been produced from eggs laid by fome Fly upon the difeafed part, and I had no doubt, that from them would be produced other Flies of the fame fpecies with that which had laid the eggs. This I communicated to the furgeon, who, at firft, did not give much credit to it, as not being able to conceive how any Fly could find its way to the part to lay thofe eggs.

In purfuit of my inquiries, concluding that the piece of flefh on which thefe maggots were found, would very foon be confumed by them, I fupplied them with other pieces of meat, which they alfo devoured; and I continued to feed them with frefh meat until the fifth day, which was the laft day, when, preparing again to feed

them, I found, to my great furprife, that having left the box which contained them open to give them air, they had all crept out of the box, and it was not till after a diligent fearch, that I found many of them (for they had been fifty in number), in the corners and chinks of my fcrutoire: they had in thefe five days grown to fuch a fize, that each of them was as long as one of my nails; and the reafon of their quitting the box, I concluded to be, that having grown to their full fize, and requiring no more aliment, they concealed themfelves in holes and corners, in order to undergo their next transformation.

The next morning, being the firft of Auguft, one of thefe maggots, whofe body had been fharp or pointed at one end, was contracted one third in length, fo as to be of an equal thicknefs at each end, and exhibited the figure of a fmall barrel: in the afternoon of the fame day, four others of the maggots had affumed the fame fhape, and they were changed from their original white, to a yellowifh colour: the next day they became red, and fo all the maggots, from day to day, changed from a yellowifh to a red, and at length to a blackifh colour.

Two of thefe cryfales or grubs I put into a glafs, and carried about in my pocket, with intent to expedite their change into Flies, but after five or fix days, I found the heat was injurious to them, for they began to fhrivel up, and confequently, I judged, would not produce any living creature. The others I placed on a paper, covering them with a glafs, and at the end of nine days, I opened three of them, and took out of each a perfectly formed Fly, but very moift, and without any motion that I could difcover; they were inclofed in a thin membrane, befides the outfide hard fhell which contained them. I could not at firft difcover their wings, but examining them more narrowly, I perceived the wings folded in exact order on their bodies, and having feparated them from the bodies, I found them to be perfectly formed; upon opening the bodies of thefe unborn Flies, I took out of one of them a great quantity of eggs.

On the fourteenth of Auguſt, I ſaw four fully formed Flies flying about the glaſs, and that the ſhells or veſicles from which they had iſſued, had a hole at one end: upon my putting ſome ſugar under the glaſs, the Flies immediately fed upon it.

The next day all the other aurelias or grubs produced living Flies except two which I had injured in the handling, and at the ſame time I perceived many other Flies on the glaſs in my cloſet, which I concluded came from the maggots which had hid themſelves as before mentioned. I placed before thoſe Flies ſome pieces of raw fleſh, but none of them would feed on it; neverthcleſs, on the eighteenth of Auguſt, they all fed greedily on a piece of fleſh.

On the twenty-eighth of Auguſt, I opened three of theſe Flies, and took out of one of them a great quantity of oblong eggs, each of which was a twenty-fifth part larger than the eggs I had taken out of the Flies which were not hatched; and what appeared to me worthy of note was, that to each of theſe eggs was fixed an exceeding ſmall black veſſel, through which I conclude each egg had received its nouriſhment. All theſe minute veſſels aroſe out of larger and darker coloured veſſels, and thoſe again out of a much larger one; all which veſſels I therefore concluded to be arteries. Examining theſe arteries with great attention, I very diſtinctly perceived them to be formed of annular parts, like the veſſels in the lungs of animals, but theſe annular parts were ſo exceedingly minute, that, viewed through microſcopes of very great magnifying powers, they appeared as ſlender as a fine hair of one's head ſeen by the naked eye.

Though theſe Flies appeared very vigorous, yet a ſmall touch or preſſure would cauſe them to die; for happening to break one of the glaſſes in which I kept them, whereby they eſcaped, and flew about my ſtudy, though in catching them I handled them as gently as I was able, they died in a few days; ſome loſing their wings, by which I had caught them, others the uſe of their legs, and laying on their backs motionleſs for ſeveral days; and I concluded their

deaths to be occaſioned by this, that in touching them, ſome of thoſe minute veſſels might be injured, and many of the eggs depending on them be broken off, and putrefying in the body, might occaſion death. At length, on the ſeventh of September, I had only two Flies left alive, one of which had loſt a wing. Theſe I judged to be a male and a female.

On the ninth of September in the morning, I found one hundred and forty-five eggs laid, as I judged, by one Fly: ſome of theſe eggs, with a piece of dried fleſh, I put into a glaſs and carried in my pocket, the weather being cold, to ſee in what ſpace of time maggots would be produced from thoſe eggs, and I found ſome of them hatched the very ſame day. The next morning all the others were hatched, and I found that, in that one night's time, they had all grown twice the ſize of the eggs.

I again put ſome more eggs into a glaſs, and carried them in my pocket, and in five hours time they were all hatched, and in ſeven hours more they were grown to twice their original ſize, ſo that I concluded for certain, that the maggots which had been brought to me on the piece of fleſh taken from the gentlewoman's leg, had been produced from eggs laid on it at the laſt dreſſing, by ſome Fly, and that when brought to me, they had been hatched but a few hours.

I cauſed drawings to be made of the Maggot, the Grub, and the Fly, I have here deſcribed, becauſe theſe Flies are the largeſt ſort found in our country. Plate XVIII. *fig.* 21, is the maggot when grown to its full ſize and five days old. *Fig.* 22, is the cryſalis, aurelia, or grub into which the maggot was transformed, and at one end of it appears the hole through which the Fly iſſued. *Fig.* 23, is the Fly: and unleſs I had been convinced by my own experience and inſpection, it would have ſeemed incredible to me, that ſo large a Fly could proceed from ſo ſmall a grub; but we muſt conſider, that the wings, and alſo the hairs with which the Fly is covered, are placed as cloſe as poſſible to its body, while in its aurelia or cryſalis ſtate; but when it becomes a perfect Fly, they ſeparate from the body,

and riſe up at ſome diſtance from it, and conſequently appear larger than they are in reality. *

I know many people are of opinion that flies are produced from corruption, and they pretend to bring many inſtances in ſupport of that notion, which occurred to me lately in converſation with a certain learned gentleman, who argued thus upon the ſubject:

" I have obſerved," ſays he, " in a parcel of grubs or aurelias, " produced from ſome caterpillars of the ſame ſpecies, four butter- " flies produced, all of the ſame kind and ſhape, and from the fifth " aurelia, which had an aperture like the others and was tranſpa- " rent within, three common Flies iſſued. The cauſe of this ap- " pearance I could not account for."

To this gentleman I made anſwer, that theſe reaſonings of his made no difficulty with me, becauſe I conceived the matter might be accounted for as follows:

Flies, and almoſt all living creatures which are not able to nouriſh their own young, have it implanted in them by Nature to lay their eggs in thoſe places where the young, when hatched, may find food. When, therefore, Flies of any description cannot find any fleſh, fiſh, or offal, they often lay their eggs in thoſe places where their inſtinct informs them their young will find ſubſiſtence, and this is in the grubs or aurelias of caterpillars; the maggots hatched from theſe eggs laid by the Fly can eaſily perforate the thin coat or caſe of the aurelia, and uſe for their nouriſhment that ſubſtance within it, which was deſtined to the formation of a winged creature of a different ſpecies, ſo that from ſuch an aurelia a Fly inſtead of a butterfly may be produced. With this argument of mine the gentleman declared himſelf to be ſatisfied.

Now, I lay it down for a certain truth that it is equally impoſſible for a Fly, or other living animal, to be produced from corruption, as for rocks to bring forth horſes or other beaſts.

* Another reaſon may be aſſigned for this appearance, namely, the rapid growth of flying inſects immediately after their coming forth from their aurelia ſtate. See the Tranſlator's remark on this ſubject in a note, Vol. I. p. 28.

Many perſons cannot ſufficiently wonder at the immenſe quantities of Flies with which the inhabitants of a beſieged town, of any note, are infeſted. But we may eaſily ſolve this difficulty, when we conſider that it is impoſſible for the commanding officers to cauſe all the bodies of the ſlain to be interred, and that from them, and from the entrails and offal of beaſts, left expoſed in the fields, the number of Flies muſt increaſe beyond meaſure. For, let us ſuppoſe that in the beginning of the month of June, there ſhall be two Flies, a male and a female, and the female ſhall lay one hundred and forty-four eggs, which eggs, in the beginning of July, ſhall be changed into Flies, one half males and the other half females, each of which females ſhall lay the like number of eggs; the number of Flies will amount to ten thouſand: and, ſuppoſing the generation of them to proceed in like manner another month, their number will then be more than ſeven hundred thouſand, all produced from one couple of Flies in the ſpace of three months.

144	Flies in the firſt month.
72	of which ſuppoſed females.
144	eggs laid by each female.
288	
288	
72	
10368	Flies in the ſecond month.
5184	of theſe females.
144	eggs laid by each female.
20736	
20736	
5184	
746496	Flies in the third month.

Conſidering this we need not wonder at the great multitudes of Flies obſerved where the bodies of great numbers of men or animals lie unburied.

There is a wonderful circumſtance, and well worthy of note, in regard to Flies, namely, that the maggot from which a Fly is produced will come to its full ſize in the ſpace of five days: for, if a month or more was required for this purpoſe, as is the caſe with other maggots, it would be impoſſible for Flies to propagate their kind in the heat of ſummer, becauſe the Fly's maggot can ſcarcely ever have any food than what is found in the place where the egg was firſt laid. Now this food of theirs, namely, fiſh, fleſh, or offal, lying in the open air and expoſed to the ſcorching heat of the ſun,

would continue but for a very few days to be fit food for the maggots, therefore the All-wife Creator has implanted in thofe maggots the property of acquiring their full growth in a very few days, when, on the contrary, other maggots which can have a continual fupply of food, are months before they undergo any alteration.

I have, at times, carried feveral of thefe maggots about me in a glafs, giving them every day a fupply of flefh, and fhewed them to feveral curious perfons, that they might with me obferve their wonderfully rapid growth; I have, indeed, brought them to their full fize in the fpace of four days, fo that I conceive in the heighth of fummer the eggs laid by Flies may, in lefs than a month's time, become complete Flies, fo as again to lay eggs. Laftly, it is worthy of obfervation that thefe maggots do not void much excrement, fo that the greateft part of the fubftances they confume for food enters into the compofition of their own bodies.

AT the time I firft turned my thoughts on the nature of our common ftinging Nettles, I imagined that the great pain and fwelling they occafion arofe from the fharp points of the ftings or prickles, which are thick fet on their leaves and ftalks, being broken off and left within the fkin: but happening one day, while gathering afparagus in my garden, to be ftung between my fingers by a very fmall Nettle, it produced fo uncommon a pain and fwelling, that I examined more narrowly the formation of Nettles by the microfcope, and I found that the ftings or prickles are not only hollow, and contain within them a very tranfparent juice, but that, at the time when they are in their moft vigorous growth, this juice iffues from the ftings, and may be feen to fettle on the points in the fhape of a very fmall drop or globule.

Upon feeing this, I formed a different opinion on the fubject, and I conceived that, though we may be pricked by the Nettle no deeper than the external cuticle or fkin, and though the point of the fting may not be left behind, yet we fhall experience both pain and

ſwelling, if the liquid, which is at the extremity of the ſting, or can by any means be expelled from thence, penetrates within the ſenſitive part of the ſkin, and there touches or wounds any of the veſſels; whereupon ſome acute ſalt, which this liquor contains, principally produces the pain and ſwelling we experience; and this I rather take to be the caſe, becauſe, on examining Nettles which had paſſed their full growth, I found that the juice in many of the ſtings was dried up, whereas thoſe that were ſtill growing were not only quite full of juice, but ſome of it iſſued from their points as before mentioned. And I obſerved the points of thoſe which had come to their full growth, to be for the moſt part broken, which I attributed to the wind agitating the leaves, and ſtriking the ſtalks one againſt another.

I know many people ſay, that if we boldly graſp a Nettle it will not ſting, but the only reaſon is this, that if we ſeize a Nettle with the whole hand our fingers are cloſe together, ſo that the Nettle only touches the ſkin on the inſide of our hands and fingers, which is generally ſo thick and tough that the ſtings cannot pierce it, but are either blunted or broken, and therefore we feel no effect on the inſides of our fingers, much leſs the palms of our hands; but the parts between our fingers, where the ſkin is thin and ſoft, are thoſe liable to be injured.

In order to ſhew the formation of the ſtings of the Nettle, I have cauſed drawings to be made of them from the microſcope.

Plate XVIII. *fig.* 24, A B C D E, is the ſting of a Nettle, as it grows on the leaf or ſtalk, when in its moſt flouriſhing ſtate. At C is to be ſeen a round drop or globule, being part of the juice with which the cavity of the ſting is filled. A B D E is a ſoft, green, flexible part, which has this appearance while on the plant, but, when ſeparated from it, in a little time more than half of it dries away. B C D is the ſting itſelf, which is very tranſparent when filled with juice.

Fig. 25, F G H I K, is the ſting of a Nettle in its vigour, at the end of which, H, no juice is emitted.

Fig. 26, L M N O P, is the ſting of a Nettle come to its full growth, and in which the moiſture which it contained in its cavity is dried up, as is ſeen between M N O; and as in this ſting we ſee ſeveral particles, which in the figure are marked 1, 2, and 3, I imagine that this evaporation of the juice from the ſting was not performed in one day, but at ſeveral times, one day more than another, according to the heat of the weather. Between 3 and N, this ſting appeared very dark and of a greeniſh colour, which doubtleſs was the ſolid ſubſtance of the juice in the ſting there coagulated.

Fig. 27, Q R S T V, is the ſting of the Nettle cut tranſverſely to ſhew the cavity within, as appears at the letter S; and Q R T V is the green and ſoft part dried up thus far.

Fig. 28, W X Y Z, is a ſting of the Nettle cut off nearer the point, where the letter Y indicates the cavity.

Of the SHRIMP.

I HAVE often reflected on the nature of fome forts of fhell-fifh, fuch as Shrimps, Lobfters, and Crabs, in this refpect only, that they bear their multitudes of eggs on the outfide of their bodies, which, if we reflect, muft neceffarily be fo, for otherwife it would be impoffible for thofe fifh to produce fo great a quantity of young as we find they do, becaufe their bodies being covered with an hard fhell, cannot be diftended, and would not allow fpace for the growth of their eggs, if they were always within their bodies.

The eggs of our Sea Shrimps are not produced and brought to maturity at any one particular time of the year as is the cafe with moft other fifh; for I obferve Shrimps to have eggs at all times of the year; I fpeak here only of thofe Shrimps which are caught near this coaft; for I am told that the Shrimps about Amfterdam, at fome feafons, are all loaded with eggs, and the remainder of the year are deftitute of them.

But what I obferved beyond my expectation, and which is well worthy of note is this: that every Shrimp's egg, when come to maturity, contained in it nothing but a complete minute Shrimp, which not only I could fee lying in the egg in a circular pofition, with the tail turned over its head and laying on the back; but, when taken out of the fhell, I could fee fome of its fcales, together with the tail and fins; alfo the head and eyes, and thofe limbs or organs which iffue from the head, and which lay as it were folded together near the feet. In fhort, Shrimps are completely formed in the eggs before they are feparated from the parent.

Thefe eggs, for the reafon I have mentioned, not being capable of expanfion within the body of the Shrimp, are carried about by

the parent, being fixed between thofe organs or limbs, which we call its feet, and there they are nourifhed and brought to full maturity by a fmall ftring or ligament; being thus protected by the parent until the young Shrimp within is able to break through the fhell and feek food for itfelf.

In order to view the nature of thefe unborn Shrimps, I took feveral out of the fhells, and having placed them before the microfcope, I fent for the limner, whom I directed to make as accurate drawings of them as he could, without informing him what the objects were, and while he was employed in fo doing, he often faid, "I do not know what I am drawing, but it feems to me to be a "Shrimp."

Plate XVIII. *fig.* 29, A B C D, is one of thefe unborn Shrimps, taken out of the egg, which I extended as much as poffible from the rounding pofition in which it lay; but though I endeavoured to take thefe Shrimps out of the eggs with the greateft care, yet I could not do it fo completely, but that in one of them the tail appeared moft plainly, in another the head, and in another the feet. In this figure, A B D denotes the head of the Shrimp, but wherein the feet and other prominent parts, which we fee in full grown Shrimps with the naked eye, cannot be diftinguifhed, becaufe all thofe parts are, as it were, clofely folded together next the head, and are extended towards B. *Fig.* 30, E F G H I K, is another unborn Shrimp, which drawing I caufed to be made only becaufe here, at the letter E, the fins can be plainly feen, together with the bony parts deftined to give ftrength to the fins. E F G is a part of the yolk of the egg yet remaining.

Upon comparing the fize of the eggs from which thefe young Shrimps were taken, with grains of common fcowering fand, I found them, for the moft part, of the fame fize, though fome were rather larger and others fmaller.

Some years after this I renewed my obfervations on Shrimps, in order to difcover the circulation of the blood in thofe animals, and

in doing this my eye was taken with thofe two protuberant parts, which are placed in the fore part of the Shrimp's body, and which are commonly called its head. Thefe protuberances are the eyes of the Shrimp, which it has a power of moving in every direction, fo as to view all furrounding objects.

Having taken out the tunica cornea or horny coat of thefe eyes, and well cleanfed the infides of them, I found that the tunica cornea in this creature was very foft and flexible, in comparifon with thofe in the eyes of flies; and when placed before the microfcope, in order to view objects through them, I found that fuch objects appeared with much lefs diftinctnefs and brightnefs than when viewed through the optical organs of a fly. The reafon of this difference I took to be, that in Shrimps, and many kinds of crabs, but more particularly in Shrimps, they, being always immerfed in water, do not require to be hard or tough, and, therefore, when expofed to the air and becoming dry, they contract; infomuch that thefe eyes which, in their natural ftate and united to the animal's body, are of a fpherical figure, when dry lofe that fpherical fhape, and from being convex become concave, by contracting inwardly.

I caufed a drawing to be made of one of the eyes of the Shrimp, the fame fize as it appears to the naked eye, and this may be feen at *fig.* 31, X. *Fig.* 32, A B C D, fhews a great part of the Shrimp's eye, as feen by the limner through the microfcope, and here may be feen a number of optical organs in that fingle particle or fpot which we commonly call the eye. But, as in many flies each optical organ feems to be furrounded with a kind of border of fix fides, here, in the Shrimp, each optical organ is contained in a kind of fquare.

In the figure, that which in every optical organ is reprefented by a fmall circle, is to denote a cavity or finking, but which originally was of a perfect protuberant fperical figure, until the optical organs in the Shrimp, upon drying, became contracted.

Fig. 33, E F G H I, exhibits another large portion of the Shrimp's eye, placed in a direct line oppofite to the fpectator. Between the

letters E F G K appears a kind of mufcular part, void of optical organs, and this part I concluded was what ferved to move the eye.

Farther, I concluded for certain, that in like manner as every optical organ in the eye of a fly has its optic nerve, fo in the eyes of Shrimps every optical organ muft have its proper optic nerve. I therefore opened many of the eyes of Shrimps, and not only difcovered a great number of minute optic nerves, but alfo a great quantity of pellucid particles, every where mixed with thofe optic nerves. Hereupon I began to confider whether or no thofe pellucid particles (which often appeared to me rather oblong and fomewhat crooked) might not each of them be the cryftalline humour of one fingle and individual optical organ, and that their being of the oblong and crooked fhape I have mentioned, might be occafioned in the diffection of the tunica cornea, whereas thofe particles were, in their natural ftate, of a perfectly fpherical fhape, and filled the whole internal cavity of their refpective optical organs. And, in this opinion I was confirmed when, upon more carefully attending to the Shrimp's eye, I perceived the tunica cornea to have a whitifh appearance, and the optic nerves, in the parts towards the infide of the head, to be of a dark appearance, and, when feparated from each other, exhibited a violet colour. I alfo concluded, that if it were poffible to take out the eye of a Shrimp, in fuch a manner as to preferve the cryftalline humours within the cavities of the tunica cornea in their perfect fhape, uninjured, I might place them before the fight, and that, doubtlefs, I fhould fee the objects through them as clearly and diftinctly as when viewed through the eyes of flies.

In the next place, I was defirous of knowing what was the food of the Shrimp, and I had obferved that the part in thefe creatures, which we call the head, and which when we eat them we throw away, was not only the head, but, for the greateft part, the belly of the Shrimp, whence I concluded that there its ftomach might be found.

The firſt thing I examined was the mouth of the Shrimp, which I found on all ſides provided with various organs, and each of theſe again furniſhed with many weapons, the whole of which ſeemed contrived for the purpoſe of catching the Shrimp's prey, and conveying it to the mouth.

Upon opening the mouth I ſaw that it had on each ſide a tooth : the fore part of this tooth, to be uſed in biting, was not ſmooth or even, but made with indentings or notches ; and, upon applying one tooth to the other, I found that the riſing parts in one tooth moſt exactly fitted the cavites or notches in the other. Theſe teeth in the fore parts or edges were a little broader or thicker and of a yellowiſh colour, but the lower ends of them were white.

I cauſed a drawing to be made of the Shrimp's tooth, as ſeen by the microſcope, and this is ſhewn at *fig*, 34, K L M N O, in which figure the letters K L M denote that part of the tooth chiefly uſed in biting.

Coming to the ſtomach, which I took out of the bodies of ſeveral Shrimps and laid open, I ſaw that ſome of the animals had fed on ſmall fiſhes, mixed with minute Shrimps bitten in pieces, and in one Shrimp I found ſome large fragments of another Shrimp which had been of a middling ſize, from whence it appeared plain to me, that a Shrimp can open its mouth much wider than I had before imagined.

The ſtomachs of other Shrimps were empty ; many others I ſaw containing a great quantity of thin particles, which I deemed to be ſmall bones, which had been part of the fins of ſome ſmall fiſhes, and alſo ſome of the organs of other Shrimps ground to pieces in the mouth.

Theſe thin oblong particles I have mentioned, when viewed by the microſcope, exhibited the ſame appearance as the fragments or ſhavings of one's beard viewed with the naked eye, with this difference only, that theſe particles terminated in a point at one end. I alſo ſaw a particle, one ſide of which was armed with teeth notched

like a ſaw; and I alſo ſaw the claws or pincers of a young crab, which had been no larger than a grain of ſand.

The ſtomachs of others were filled with fragments of ſmall bones from the ſpine of minute fiſhes, appearing like the back bones and tails of haddocks. I likewiſe ſaw a ſmall bone of a ſhape and make different from any I had ever before ſeen in fiſhes.

I alſo ſaw in the ſtomachs of many Shrimps, but not in equal quantities in all, ſmall ſhells, in ſhape ſimilar to thoſe ſhells which are found in the greateſt quantities on our coaſts, nor did I find many fragments of ſuch ſhells, whence I concluded that, about the beginning of September (for it was about that time theſe obſervations were entered upon) many of the ſmall ſhell-fiſh, newly produced from the parent, had been caught by the Shrimps, and their ſhells broken by their teeth, in order to pick out the young fiſhes from the ſhells and uſe them for food.

Many of theſe minute ſhells were no bigger than large grains of ſand, and I was moreover well aſſured that all thoſe ſhells which I took out of the ſtomachs of the Shrimps had been but a few days brought forth by the parents, becauſe many of thoſe ſhells were quite tranſparent, others of them were in part about their outer edges rather obſcure, by reaſon, as I imagined, that they had had an addition in growth ſince the fiſhes were brought forth.

I cauſed a drawing to be made of one of theſe minute ſhells, but which was one of the largeſt of thoſe I had extracted; and, at *fig.* 35, this ſhell is ſhewn of the ſame ſize as it appeared to the limner by the naked eye. *Fig.* 36. P Q R S, is the ſame ſhell ſeen through the microſcope: the letters P Q R T ſhew that part of it which appeared bright and tranſparent, ſo that all without the letters PQRT which was not tranſparent, was added to the ſhell by growth, after it was ſeparated from the parent.

In ſome of the Shrimps' ſtomachs I found ſome exceeding minute ſhells, which, on account of their round figure, I called ſnails, and which were not larger than a large grain of ſand; and to ſhew the

pretty ſhape of theſe ſhells, I thought it would not be amiſs to give a drawing of one of them: this is to be ſeen at *fig.* 37, A B C, as viewed through the microſcope.

I next examined the inteſtines of the Shrimp, to ſee whether I could there diſcover any of thoſe ſmall bones or ſhells, but finding none, I began to conſider that, perhaps, when Shrimps have kept thoſe ſhells, or other hard ſubſtances, which cannot be digeſted or ground to pieces by the action of the ſtomach, ſo long as that all the nutrive particles which can paſs into the inteſtines are extracted, whether they do not throw up the remainder, and void it at their mouths.

The paſſage out of the ſtomach was provided with certain organs, like teeth, for the purpoſe (as I think) of grinding the food a ſecond time, before tranſmitting it to the inteſtines.

I have often reflected on the great number of eggs which Shrimps are accustomed to carry, faſtened to thoſe limbs or organs placed on the lower parts of their bodies, and which are commonly called their feet; and eſpecially when I conſidered that thoſe eggs, which are by ſome means fixed on that part, did not ſeem to grow any larger, and that all thoſe which I ſaw were of the ſame ſize, from whence I could not ſatisfy myſelf in the belief that thoſe eggs had been formed in that place.

Hereupon I began to diſſect the belly of the Shrimp, which is in that part commonly thought to be its head, and then I ſaw that the eggs were chiefly placed there, and the remainder of them next the back, where the body of the Shrimp is thickeſt.

The eggs which I took out of the bodies of Shrimps were not of equal ſize in all, but ſome larger than others: and beween thoſe eggs which had been put forth from the Shrimp's body, and were fixed to the lower part of the body, and the largeſt of thoſe which ſtill remained within the body, I could not diſcover any difference in point of ſize.

It was to me a pleaſant ſpectacle to view the eggs, as they lay in the bodies of the Shrimps, for every one of them had in the middle of it a ſmall round bright ſpot, in like manner as if we were to ſee with the naked eye, a tranſparent egg, with a ſtill more tranſparent yolk in it.

Having made theſe diſcoveries, I concluded for a certainty, that when the eggs within the Shrimp's body are come to their full ſize, that they are ſoon afterwards emitted out of the body, and, by the Shrimp itſelf, fixed to thoſe organs commonly called by us its feet, and that they are alſo placed as cloſe as poſſible to the animal's body, ſo as to be no impediment to thoſe organs in the performance of their uſual functions. But though each of theſe organs is furniſhed with a number of ſtill ſmaller organs, yet all taken together would not be ſufficient to hold faſt and ſecure the great number of eggs with which they are loaded, nor could theſe eggs be ſecurely faſtened, unleſs each egg had a kind of ſtring or ligament, by means of which they are all faſtened together, like a chain or ſtring of beads.

Fig. 38, D E F, repreſents a very ſmall portion of theſe eggs, which were dried and placed before the microſcope after being taken out of the body of the Shrimp.

Fig. 39, G H I K L, repreſents the receptacle of the egg, of its natural ſize, taken out of the Shrimp: the letters G H K L indicate that part which was in what is called the Shrimp's head, and H I K that part which lay in or next to the ſolid part of the Shrimp's body. Now, when theſe eggs increaſe in ſize, then that part, marked G L, in part ſurrounds the ſtomach of the Shrimp, and, in proportion to the quantity of food in the ſtomach, it either extends farther or is repreſſed by the diſtention of the ſtomach.

I have alſo obſerved that, when diſſected, ſome Shrimps whoſe eggs which were fixed on the outſide of their bodies had arrived at that maturity that, I judged, they would produce young minute Shrimps in a few days, within the body of the ſame Shrimp I could perceive

a great number of eggs, which I thought were grown to that ſize, that within a few days they would be emitted from the body and placed on the outſide of the Shrimp.

As we now ſee that thoſe Shrimps, which are taken in the ſea near our coaſts, do faſten their eggs to the lower parts of their bodies until the young are able to iſſue forth and ſwim about (whence it is, that Shrimps newly hatched can, in ſtormy weather, retire to the deeper water, and avoid the agitation or ground ſwells of the ſea upon the ſhore); on the contrary, among thoſe Shrimps which are caught in the inland ſea, round about Amſterdam, few or none are found bearing their eggs about them in that manner: hereupon it ſeemed probable to me, upon reflection, that perhaps theſe laſt mentioned Shrimps, when their eggs are grown to a ſize to be excluded from their bodies, reſort to the creeks and ſhallows of that ſea, where they can find a ſufficient quantity of leaves and ruſhes to which they can faſten their eggs.

In the month of May I examined the bodies of ſixty-four Shrimps, forty-nine of which had eggs fixed to the lower parts of their bodies, and the young ſhrimps in theſe eggs were ſome of them more perfectly formed than others, and the darker the colour of the eggs the more full grown were the young within: of the remaining fifteen, I found eggs within the bodies of twelve.

I oftentimes repeated theſe my reſearches, and always with nearly the ſame ſucceſs. And when I met with Shrimps which had no eggs faſtened to the outſides of their bodies, I began to conſider whether or no thoſe Shrimps might not have faſtened their eggs to leaves, litter, or other ſubſtances at the bottom of the ſea.

I afterwards examined many parts of the inteſtines of this creature, which were formed ſo wonderfully, and with ſo much art, that to attempt a particular deſcription of them would be only loſing time and labour. In a word, if we could place all thoſe organs before the microſcope, make drawings of them, and could aſſign to each part its uſe, we might cry out, What depth of wiſdom is here!

what hidden wonders in fo contemptible an animal! and, how little is it that we know!

The mouth of the Shrimp is not, in my judgment, calculated to catch fifhes, but only to pick up its food, but that defect is well fupplied by two organs placed on the fore part of the Shrimp's body, and which may be compared to arms and hands; thefe are furnifhed with many joints, and at the extremities have each of them two nails or claws, the larger of which is moveable, and the fmaller fixed and immoveable: by the help of thefe claws the Shrimp can, in my opinion, catch and hold faft many minute fifhes, and alfo convey them to its mouth, there to be ground fmaller by the teeth.

I have given a reprefentation of thefe claws, as feen by the microfcope: *fig.* 40, MNOPQRST, is one of them, which I may call the Shrimp's hand. The part denoted by the letters NOP is the nail, which I think is immoveable, and only ferves the Shrimp to pierce into the bodies of fuch fmall fifhes as it catches. PQ is a fharp bony part, which ferves to hold the prey more firmly, and is covered with fhort hairs, fome of which extend beyond the edges of it. QRS is the larger claw, which the Shrimp can open and fhut at pleafure, and it is hollowed on the infide, the more firmly to keep its hold. *Fig.* 41, VWXY, reprefents the fame organ, which I call the Shrimp's hand, in fuch a pofition that the larger claw is clofed: VW reprefents the fmaller claw, and VXY the larger, which, when clofed, reaches a little beyond the fmaller, as may be feen at V.

I have fometimes feen thefe claws broken, and in a ftate of decay at the points, caufed, as I fuppofe, by their ftruggling with fifhes too ftrong for them.

From the view of thefe claws in the Shrimp I was able to fatisfy myfelf as to the ufe of thofe very ftrong claws which we obferve crabs to be provided with, and which, I think, are not intended

merely for catching their prey, but alſo for grinding or breaking it into ſmall pieces, before it is taken into the mouth.

I have obſerved Shrimps to have eggs on their bodies during the whole of the ſummer, but in the month of November no eggs were to be found on them. But when we conſider, that all the time Shrimps carry eggs on the outſides of their bodies, other eggs are forming within them, who can pretend to ſay how many times in a year Shrimps bring forth young ?

I never was able to ſatisfy myſelf that I had found a Shrimp of the male ſex ; and all the pains I could take in the ſearch more and more convinced me that there are no males among theſe creatures, herein reſembling thoſe ſmall flies which I have diſcovered, and deſcribed in another place.

Theſe things being conſidered, namely, that all Shrimps indifferently do bring forth young, and that many times in the year, and every time in very great quantities : theſe conſiderations, I ſay, fully ſolve what was once with me a difficulty, in regard to the great quantities of Shrimps which are continually brought for ſale, inſomuch that many perſons in our large towns make a living by that traffic, beſides the people who are employed in taking the Shrimps in the ſea.

Of the Salts contained in Pepper, and in Tea, with the Author's reaſonings thereon: the Salts found in Cantharides deſcribed.

I HAVE often reflected on the nature of Pepper, and particularly what might be the reaſons that the particles of Pepper excite ſuch a pungent ſenſation in the mouth, when the ſame, taken into the ſtomach or inteſtines, do not cauſe any irritation, ſo as to promote an evacuation.

I at one time thought, on looking at the internal or mealy part of Pepper, that the particles compoſing it might many of them be very ſharp pointed, and thereby produce that kind of pricking on the tongue, which many call heat or burning. But I afterwards rejected this opinion, becauſe, upon contemplating a great number of theſe particles, I ſaw they were all of different ſhapes.

I made a drawing, from the microſcope, of five of theſe particles, many of which together conſtitute one grain of Pepper, and theſe are ſhewn in Plate XIX, at *fig.* 1, A. Many of theſe particles are, indeed, very long and ſharp pointed, which I never obſerved in any other ſeed I have examined, for the mealy particles in other ſeeds are more of a globular form. Beſides being of an oblong ſhape, the particles of Pepper are flat on ſome of the ſides and irregular on others, but in ſuch manner that there are no cavities between the particles, for otherwiſe each grain of Pepper, when come to maturity, would not be a ſolid body.

But what is very remarkable, theſe ſmall particles of Pepper, when laid in water, do neither ſwell nor become ſoft, like the mealy ſubſtance of wheat, rye, peaſe, beans, and the like, but preſerve their ſhape (at leaſt as far as I have been able to obſerve).

Many of theſe particles of Pepper are tranſparent like glaſs, and others of them may be ſeen to be compoſed of ſtill ſmaller particles.

Having examined the parts of common Pepper, I took ſome of that which is called long Pepper, and becauſe I believe that many perſons are unacquainted with this kind of Pepper, I have cauſed a drawing to be made of it, which is repreſented at *fig.* 2, B C D E, in which B C is the ſtalk, and C D E the fruit or Pepper itſelf.

Upon cutting open this long Pepper, I perceived it was not a ſingle ſeed of Pepper, but what is called a pericarpium, that is, a caſe or ſhell containing many ſeeds, and in this ſhell were contained above an hundred ſmall grains of Pepper,

Several of theſe ſmall grains, each of which is contained in a particular ſkin or covering cloſely adhering to it, I took out entire, and held them in my mouth, in order that by the moiſture the coverings or ſkins incloſing them might be ſoftened, and I might the eaſier diſcover the mealy ſubſtance of which theſe ſmall grains of Pepper are compoſed. In doing this I found that the grains, though not broken, excited as ſtrong a ſenſation on my tongue as if they had been pounded Pepper, whence I concluded that the mealy ſubſtance of theſe grains, which in ſize and ſhape agreed with our common Pepper, did not excite that ſenſation by the acute particles in them, which I have ſaid are to be diſcovered by the microſcope, but rather by ſome ſaline particles exciting on our tongues that kind of pricking which is called heat.

I therefore took ſome of this long Pepper, which I put in clean paper and pounded it on an anvil: I then put it into a new glaſs veſſel, pouring rain water on it till it was covered to about the third part of an inch; after this water had ſtood about two hours I poured it off, but it being evening I let the water ſtand all night; the next morning I ſaw, in the place where the water was moſt evaporated, an incredible number of ſaline particles, many of which were almoſt twice as long as broad, but one ſide always longer than the other, both ſides parallel, and the two ſhorter ſides ſloped from the

longer, as reprefented at *fig.* 3, F. All thefe faline particles were exceedingly thin. There were many others formed in the fame manner, but rather narrower, as at *fig.* 4, G: I alfo faw many faline particles in which I could not difcover any acute angles, as *fig.* 5, H; but the greateft number of particles, being more than all the reft put together, were broad in the middle and drawing to points at the ends, as reprefented at *fig.* 6, I. Thefe laft particles were of various fizes, and fome fo minute as to be fcarcely diftinguifhable.

After the water had ftood a whole day and night, I faw fome faline particles collected together in an irregular form, fo that they had two, three, or more points at the extremities, as appears at *fig.* 7, K, which I concluded was caufed by fome of the particles adhering together.

From thefe obfervations I concluded that the heat which we feel in our tongues from Pepper, is nothing but the falts or faline particles paffing from the Pepper to the tongue, and the fharp points of which fo prick the tongue as to excite the fenfation we call heat, as before mentioned; but when thefe faline particles by degrees coagulate, as I have before obferved, and get into the ftomach and inteftines, they may, by fuch coagulation, fo alter their fhape as to be prevented exerting fuch a ftimulating power as to caufe evacuation, or at leaft thefe particles may alter their figure; to which we may add that the juices in the throat, ftomach, and bowels, may promote fuch coagulation, or other difpofition of the pungent faline particles, as to prevent their irritating the ftomach or inteftines, if they are not taken in too great a quantity.

To fatisfy myfelf farther on this fubject, I took a new glafs and put into it fome long Pepper, and, by applying fire to it, I drew off oil from it; the oil, or fpirit, which was firft driven off by the fire, I often examined, but faw nothing in it worthy of note; the oil that came off laft, being heavier, funk to the bottom, but in this I remarked nothing particular.

I afterwards mixed water with the oil, and by the fire I mixed the oil and water as much together as I was able, but in this I could obferve nothing except a few faline particles, like thofe reprefented at *fig.* 6, I; but thefe I could not well diftinguifh on account of the oil, the water being evaporated. I alfo faw in the oil various particles, which I thought were falts irregularly joined together.

Upon the caput mortuum, or fubftance left behind by the fire, I poured fome clean rain water, and let it ftand for fome hours, that the fixed falts it contained might be drawn off by the water; after this I poured off the water as clear as I could and expofed it to the air in my clofet, the weather being calm and the fun fhining bright, but the water did not at firft evaporate: but, after ftanding two days and nights, I faw a great number of faline particles fwimming in the water, like thofe pictured at *fig.* 6, I. I alfo faw many fmall flat particles of fix fides floating about, fome of which were fo fmall that I could not diftinguifh the fhapes of their fides without great attention. I concluded that this liquor was, for the moft part, compofed of fixed falt; therefore I expofed it to fome greater degree of heat, and then I faw, as I may fay, the whole of it converted into irregular faline particles, which were as tranfparent as glafs, and in fuch numbers that they looked like a heap of fand.

Upon breathing my warm breath on this newly formed falt it again changed into a tranfparent fluid liquor.

From thefe obfervations I neceffarily concluded that there was but little volatile falt in Pepper, becaufe I obferved few particles of that fort in it.

After this I took common white Pepper, becaufe I think that there is no other difference between white and black Pepper, than that the outward fkin or fhell, which is ftripped off the former, is left on the latter.

This Pepper I alfo put into paper and pounded it into fmall parts, and infufed it in water. After the water had ftood about two hours I poured it off as clear as I could, and in the evening placed it in my

clofet: the next morning I examined it, and with great pleafure I faw in it all the fame kinds of faline particles as I have pictured at F G H I, thefe I could difcern moft diftinctly, and in great numbers, yet much fewer than thofe I had before feen in the long Pepper, though the quantities of white and long Pepper, and of water, in which I had infufed them both, were nearly equal.

I alfo drew off the oil and fpirit from white Pepper, but I could fee nothing in the oil except a few particles, which I thought to be falts, in the thinner oil or fpirit I faw fome faline particles, the fame as reprefented at *fig.* 6, I. Thefe were fo fmall, and fo hid by the oil, that they could not be diftinguifhed without great attention.

Farther, I poured fair rain water on the caput mortuum, or folid fubftance left of this Pepper, and after it had ftood for fome hours, I poured it off; and when the greater part was evaporated, I faw fome faline particles lying in it which had fix fides like an equilateral triangle, with the three angles or points a little cut off, as fhewn at *fig.* 8, L. Others had fix equal fides: there were alfo fome exceeding thin quadrangular figures, as at *fig.* 9, M, and fome parallelograms, as at *fig.* 10, N, which were wonderfully thin. Alfo many fmall particles like thofe pictured at *fig.* 6, I, fome of which were blunt at one end, and fharp pointed at the other. I faw alfo various particles, at the extremities of which I could not difcover any point. All thefe faline particles were furrounded with an exceeding thin tranfparent liquor, which upon the leaft heat being applied, was changed into irregular faline particles, which caufed all the before mentioned particles to alter their exact fhape. But with the fame eafe as this liquor was converted into falts, it was again refolved into a fluid only by breathing upon it twice.

A gentleman of my acquaintance received from the Eaft Indies, among other prefents, a fmall jar filled with the falts extracted from

Tea, which he confidered as a prefent remedy againft fevers. A parcel of this he gave to me, which having left for fome hours on a fheet of paper, I found it foft, as if it was going to melt away, therefore I put it into a new glafs, and poured rain water upon it, in order to diffolve it entirely.

I expofed three different portions of this water to the air, that part of it might evaporate, and by this means fome particles of falts became vifible, and after a few hours were expired, I faw a great number of particles fwimming in the water, which were almoft all of the fame fhape, namely, oblong, and terminating obliquely at the extremities, as at *fig*. 11, O. But thefe figures were not flat, but fomewhat rifing in the middle in a fort of ridge, and fharp at the ends, and tranfparent like cryftals. Others of thefe faline particles were not fo perfect, becaufe one half of them was fomething broader than the other half, as is fhewn at *fig*. 12, P.

After this water had ftood about two days, I faw the faline particles in the place where the water was not entirely evaporated to be fomewhat increafed in fize, but retaining their former fhape.

I faw alfo in other parts of the liquor not evaporated, a number of fmall roundifh particles, in which I could not diftinguifh any exact fhape, though I viewed them very narrowly and with a very deep magnifier, and befides them an incredible number of wonderfully minute particles, the fhapes of which I was ftill lefs able to diftinguifh: thefe laft particles I concluded to be fo minute, that a thoufand millions of them together would not equal a large grain of fand. However, upon ufing my fharpeft and greateft magnifying microfcopes, I faw the faline particles, which, as I have faid, appeared round to me, to be, for the moft part, of fix fides, and fome few of them three. And among the minute particles that I faw in fuch incredible numbers, I was able to diftinguifh many of them to be of the fhape reprefented at *fig*. 3, F. Others were fo fmall, that I could not with certainty affign any particular figure to them.

After this liquor had ſtood more than two days expoſed to the air in my ſtudy, the weather being very warm, with a dry eaſt wind, and about half the moiſture remained, I concluded that it then moſtly conſiſted of ſalts, which were too ſoft to coagulate, I therefore applied it to a moderate heat, and then I ſaw an incredible number of ſaline particles formed in it, many of which were of ſix, and others of four ſides. But whereas our common ſalt, diſſolved in water, when it again forms itſelf into ſalts, aſſumes an exact quadrilateral figure, or if a little of an oblong ſhape of four ſides, yet each angle is always a right angle, or contains ninety degrees, as far as the eye can judge; on the contrary, thoſe ſaline particles I am now deſcribing, were not only compoſed of irregular angles and ſides, but their ſides did riſe up in a pyramidical form, like our common ſalt, ſo that they appeared to me like irregular quadrilateral cubes. But I muſt needs ſay, that none of the ſaline particles which I ever ſaw extracted from plants of any kind, appeared ſo regularly formed in their cryſtallization, or coagulation, as the ſalts produced from the firſt infuſion of Tea, and which are repreſented at *fig.* 11, O, and *fig.* 12, P.

After this, I took a parcel of my beſt Tea, which I put into a new glaſs, and, by degrees, applied to it ſo ſtrong a fire that the glaſs was red hot, collecting, with all the care I could, the ſpirit, oil, and volatile ſalt, which proceeded from it. I then obſerved that the oil, as ſoon as it was cooled, was not only very thick, but could not be rendered liquid, ſave by heat. Upon examining this oil by the microſcope, I thought that its tenacity was occaſioned by the extraordinary great number of volatile ſalts it contained, and the number of theſe ſalts was ſo inconceivable that it is impoſſible to give a true idea of them, ſo as to conceive that ſuch a ſmall quantity of Tea could produce ſo many particles of volatile ſalt. All theſe ſaline particles were of the ſame ſhape, that is, very long and pointed at both ends; theſe are repreſented at *fig.* 13, Q.

I afterwards endeavoured, for my farther ſatisfaction, to diſcover, if poſſible, how many ſaline particles would be produced from a ſingle leaf of Tea, but having reckoned up only a part of the volatile ſalts contained in one leaf, I forbore any farther obſervations, becauſe the number I had already reckoned up was ſo great that I durſt not publiſh it, as I had purpoſed to do. And, indeed, many perſons could not believe that the leaf itſelf could be divided into ſo many parts, viſible by the microſcope, as I ſaw volatile ſaline particles produced from one ſingle leaf.

Moreover, I took the caput mortuum, cinder, or aſhes of Tea, on which I poured clear rain water, in order to extract the fixed ſalt it might contain. After this water had ſtood on it one, two, and ſome of it three days, I poured it off as clear as poſſible, and expoſed it to the air to evaporate: I then obſerved in it many particles of what I before mentioned to be volatile ſalt; theſe were not only covered, as it were, with a watery moiſture, but many of them I ſaw lying in the water, ſo that I was convinced they were particles of volatile ſalt, which had been ſo cloſely united to other ſalts, and to the caput mortuum or aſhes, that the fire I had applied had not been ſufficient to expel all thoſe particles of volatile ſalt.

As I concluded that the watery moiſture which did not evaporate, and in which the before named ſaline particles lay, was nothing but a parcel of ſalts, though I could not diſtinguiſh their figures; I applied ſome heat to it, and I immediately perceived that, in fact, the moiſture was for the moſt part ſalt, for it concreted in irregular ſaline particles, and in ſuch numbers that the ſubſtance became white. But ſoon after it again changed into a fluid and tranſparent liquid, like water. But after I had ſuffered this liquid to ſtand a little longer, I perceived various ſaline particles of ſix ſides formed in it, which are repreſented at *fig.* 14, R. Among theſe, many were ſo ſmall that they almoſt eſcaped the ſight, even though aſſiſted by my very beſt microſcopes. I alſo ſaw ſome few exceeding minute ſaline particles, with figures of four ſides.

After this I, at two feveral times, examined the water which had ftood a little longer on the caput mortuum, or afhes, and then I faw a greater number of the particles with fix fides: I alfo faw a few particles like thofe reprefented at *fig.* 11, O.

Moreover, I put a parcel of Tea in cold clear rain water, and afterwards fome Tea in hot water, in the fame manner as it is commonly made for drinking; this infufion of Tea I fuffered to evaporate in part, and, in various obfervations upon it, I always found that there were formed in it many faline particles, coagulated or concreted together, as reprefented at *fig.* 13, Q, and *fig* 15, S; thefe were of various fizes, and many of them fo minute as to be almoft invifible.

Thefe faline particles could not at firft be eafily diftinguifhed, by reafon of the multitudes of fmall particles which were floating in the fluids, in fuch numbers that they made the water thick or muddy, fo that it feemed to me impoffible that hot water could, in fo fhort a time, extract fuch a number of faline particles from the Tea: and I formed an opinion that the many faline particles in Tea were of fuch a nature and property as to coagulate, or rather condenfe, the particles in water, which we call watery particles, and that moft of thofe wonderfully minute particles, which I have faid rendered the water turbid or muddy, did not derive their origin from the Tea, but from the water itfelf. Whence I concluded that all the virtue or effect of Tea, when taken into our bodies, confifts in this, that it occafions a great coagulation or condenfation of the thick particles of the chyle in the ftomach and inteftines; whence the other particles or fubftance of the chyle, which paffes to the nourifhment of our bodies, being rendered very fluid, the blood may be in a fhort time diluted or rendered thinner, by which diluting or thinning of the blood we are often relieved from pain in the head, and our whole body refrefhed.

I might here add my fentiments as to the reafons why drinking Tea is fo very falubrious to many people, though the ufe of it does not agree with fome few. But, as thefe my fentiments might not

be pleafing to fome, I have omitted them, and the rather becaufe I meet with men, whofe whole aim is to find fault with what I advance.

To thefe obfervations I will add what I have obferved refpecting Spanifh flies, or Cantharides. I have examined thefe Flies by the microfcope, and found that their feet were covered with many wonderfully fharp particles; but, as thefe are feen on common flies, I could not think that thefe particles could produce fuch an irritation that when Cantharides are mixed with any ointment, and laid on the body in the fhape of a plafter, they raife a blifter on the part.

I put, therefore, fome Cantharides into a clean paper, and, having pounded them, I poured clear rain water on them. Some of this water I expofed to the air, that the faline particles in it might coagulate, and, after about half an hour had elapfed, I faw a very great number of faline particles, which were three times longer than broad, though I could not difcover their exact fhape.

I then took a larger quantity of water, and let it ftand a whole night, to fee whether thereby the faline particles would concrete in larger figures. In this time almoft all the water was evaporated, and the faline particles were fo concreted together, that I could very diftinctly difcover the fhapes of each of them. Thefe particles lay in fuch numbers that they made the remainder of the liquid appear white; and it was a very pleafant fpectacle to me, to fee fo many particles lying together of an uniform fhape.

Many of thefe were fquare at one extremity, making two right angles; and at the other end two oblique fides, making an acute angle, as reprefented at *fig.* 16, T: others of them were oblique at one end, and the other fhaped like the preceding figure, as is fhewn at *fig.* 17, V. Some few of thefe faline particles had the two ends oblique, as *fig.* 18, W, and a few others two fides oblique, but one fide rather longer than the other, as *fig.* 19, X. The longeft

ſides of all theſe particles were parallel to each other, and the particles were very thick in proportion to their length.

Moreover, by the help of fire I drew off the oil and ſpirit from Cantharides, but neither in the ſpirit nor the oil could I diſcover any ſaline particles. After this I mixed ſome clear rain water with the ſpirit and oil, and ſhaking them together, to promote the coagulation of the ſalts, I ſaw many ſaline particles, which at firſt appeared flat and thin, and rounded at the extremities, but the longeſt ſides of them turned or rolled themſelves together, as is ſhewn at *fig.* 20, Y.

After this, I ſaw various ſaline particles floating in the ſame ſpirit and oil, of the ſame ſhape with the former but much ſmaller, at the ſame time I ſaw ſome ſaline particles of ſix ſides, but ſo minute that they almoſt eſcaped the ſight. I likewiſe perceived a very great quantity of particles, which I deemed to be ſalts, but by reaſon of their extreme minuteneſs I could not, with all my attention, aſſign any particular figures to them.

The caput mortuum or cinder left of the Cantharides, I alſo infuſed in clear rain water, in order to diſcover the fixed ſalts it might contain; and, after the water had ſtood on it about two days, I ſuffered part of it to evaporate, and then I diſcovered a great quantity of ſaline particles, the ſhapes of which I could not diſtinguiſh by reaſon of their minuteneſs. But when almoſt all the water was evaporated, I ſaw a few ſaline particles of a regular ſquare ſhape and exceeding thin, and among them a few whoſe ſides roſe up in the ſhape of a pyramid, like our common ſalt, and alſo ſome oblong figures of four ſides, ſome of them exceedingly thin; likewiſe ſome particles pointed at each end, as is ſhewn at *fig.* 21, Z, and ſome of theſe laſt ſeemed, as it were, divided or notched at the other extremity, and with two points, as is repreſented at *fig.* 22, *a.*

The little moiſture in which theſe very minute particles lay, evaporated on the leaſt heat being applied, and then ſome oblong and

rregular particles appeared coagulated. But when I breathed upon them two or three times they again diffolved into a tranfparent liquor.

I have mentioned, as my opinion, that there is no difference between white and black Pepper, except that the white is black Pepper ftripped of its fkin. And, to prove this by experiment, I took half a pound, or eight ounces, of black Pepper, which, in about three quarters of an hour, I ftripped of all the fhells or hufks, and was thereby confirmed in my opinion, for this Pepper might have been fold for white Pepper. From whence it appeared to me that white Pepper is better than black, not only becaufe it is largeft and ripeft, but alfo becaufe there is little virtue in the hufks or fhells of black Pepper, and alfo becaufe among the black there are found many parts which may be taken for grains of Pepper, when, in fact, they are only unripe grains, or only hufks of Pepper. When I had thus converted my black Pepper into white, I weighed it again, and found I had not fo much as five ounces, fo that out of eight ounces I had loft more than three, though I muft confefs I had not been very careful in the operation.

Of the young Plant difcernible in Seeds, and on the Bloffoms, Fruit, and young Wood, in the Buds of Fruit Trees.

AFTER examining the young plant in Wheat, I turned my attention to that in Rye, and after ftripping off the external fkin or hufk from the feed, I took out the young plant, and placed it before the mifcrofcope.

Plate XIX. *fig.* 23, A B C D, is the young plant in a grain of Rye, as feen by the microfcope, A a B D, are the parts from which the roots and ftems, or ftraws, will proceed; C is the top of the firft leaves in the young plant. Upon cutting this young plant tranfverfely at the end next A, I could only perceive the beginning of one ftalk or ftem cut acrofs; but when I cut a little farther towards B or D, till I came to a, I perceived two ftems, and ftill farther to B D, I faw the beginnings of three ftalks or ftems; and though I could not at firft fee more than the beginnings of three ftems or ftalks, yet I was very well convinced in my own mind, that every grain of Rye has in it the origin of four ftalks or ftems; for A a, are two principles or rudiments of the largeft young ftalks or ftems, and B and D are two other principles or beginnings: at length, after diffecting tranfverfely many grains of Rye, I perceived that I had cut through four feveral trunks, ftems, or ftalks at one cutting. All thefe young plants are placed in a kind of meal, which is not fo white as the reft of the fubftance of the Rye.

I have not given drawings of thefe young ftems or ftalks cut tranfverfely in the grains of Rye, partly, becaufe I could not place them before the limner's view fo diftinctly as in Wheat, and alfo

becaufe there was very little difference between thofe in the Rye and in the Wheat, except that thofe in Rye were much fmaller.

I alfo examined the feeds of Barley, and took from thence the young plant with its leaves. But becaufe I perceived very little difference between them and thofe in the grains of Wheat and Rye, I did not think it neceffary to give drawings of them.

But what is worthy of remark in Barley is, that in every grain there are to be feen the rudiments or beginnings of five diftinct plants, therefore I have caufed a drawing to be made of them, as they appear at one cutting of the feed tranfverfely.

Fig. 24, A B C D E F G, is a fmall piece of a grain of Barley cut tranfverfely near the end of the feed, where the origin of the young plant is formed, in which may very plainly be feen, a tranfverfe fection of five feveral particles of an oval fhape, in each of which, the veffels compofing them may be diftinguifhed. The reafon why thefe young beginnings of plants appear oval and not perfectly round, I take to arife from the evaporation of the moif-ture, as well in the plants, as in the mealy fubftance furrounding them ; for we muft confider, that the external part of the feed in drying will contract itfelf, and that when it is afterwards moiftened with water, it will, in a fhort time, again extend itfelf, and then the plants, inftead of an oval, will affume a round figure.

Thefe young plants are placed in a very fmall quantity of mealy fubftance of a darker colour, and not compofed of fuch tranfparent globules as the reft of the meal of which the Barley confifts. This dark meal is placed between G A B C D E, and between E F G is the mealy part, which for the moft part compofes the fubftance of the Barley ; and though this dark mealy fubftance may here feem to be in great abundance, yet it muft be obferved, that this does not extend farther in the Barley, rifing no higher, (in a floping di-rection), than where the young plants extend.

I afterwards examined Oats, and found that each feed contained in it the beginning, or firft rudiments, of young plants, and the be-

ginnings of leaves; and that the young plants were three in number in this feed.

Now as we fee, that the principle of vegetation, or young plant, in every one of the before mentioned feeds, is capable of producing two, three, four, or five fhoots or ftems, according to the fertility of the earth in which they are fown, we are not to wonder, that from fingle grains of Wheat, Rye, Barley, or Oats, fuch numbers of fhoots or ftraws are produced, as we obferve.

Among many other feeds which I examined, I obferved a remarkable variation in the feed of Buck-wheat, and in my endeavours to diffect this feed, I was obliged firft to take off the hard fhell, and then fteep the feed in water for feveral hours; for the leaves of the young plant were fo intimately connected with the mealy fubftance of the feed, that, until they had been thoroughly wetted, it was impoffible to feparate them. When I had taken out the young plant, I found the leaves fo twifted one among another, that it was not without great difficulty, I could fpread them apart by fmall pins, fo as to be diftinctly feen, for thefe leaves were extremely weak and tender to handle.

Having placed thefe leaves before the microfcope, I difcovered in them a vaft number of veffels, (which, in a full grown leaf, we fhould call fibres or ribs, though in fact they are nothing but veffels), appearing like what we fee by the naked eye in vine leaves. But whereas, in the leaves of the young plants of Wheat, Rye, Barley, and Oats, the veffels run lengthwife along the ftem, in like manner as the veffels do in thofe leaves when full grown; on the contrary, the veffels in the leaves of the young plant in Buck-wheat are fpread all over the leaves, in like manner as may be feen in the leaf of that plant when come to its full growth. I fhould have given a drawing of this young plant, as feen by the microfcope, were it not that it would have taken up too much room on the paper; for, (contrary to other young plants that occupy but a fmall part of the feed) thefe young plants fpread all over the

ſeed, and the leaves are extended as far as the outer ſhell or huſk of the Buck-wheat. I have, however, given a drawing of the plant ſo far magnified, as to ſhew it diſtinctly; and likewiſe a ſmall ſlice of the ſeed cut tranſverſely, in order to ſhew in what manner the young plant is placed in this ſeed.

Fig. 25, H K L M, is the young plant taken out of the ſeed of Buck-wheat, and ſeparated from the mealy ſubſtance in which it lay, and in which, as much as poſſible, the ſeveral twiſts or folds are laid open, and the whole (as I ſaid before) magnified juſt enough to ſhew the leaves and the future plant diſtinctly.

H K L, is a large and very thin leaf, within which lies another leaf much ſmaller and thinner, which it is very difficult to diſtinguiſh. This ſmall leaf, I ſeparated a little from the larger one, and let it dry in that poſition, that the limner might the better diſtinguiſh it; this is ſhewn at letter K, and the part from which the root and ſtem will proceed is noted by the letter M.

Another of theſe ſeeds I ſeparated from the hard ſhell and membrane incloſing it, and cut it tranſverſely near the point where the root of the young plant is ſituated: this I placed before the microſcope, and cauſed a drawing to be made of it to ſhew in what a wonderful manner this young plant lies among the mealy ſubſtance of the ſeed.

Fig. 26, N O P Q, ſhews the circumference of this piece or ſlice of Buck-wheat, cut tranſverſely, as above mentioned, in which N O P is the lower part of the leaf as far as it lies contiguous to the huſk, and is that part of the leaf, which in the preceding figure is ſhewn at L or H.

From P, the leaf runs inwards into the meal, and embraces or defends the half of that part from which the root or ſtem will proceed, as is denoted by the letters P V T.

The other ſide of the leaf begins adjoining the inſide of the huſk, at P, lying cloſe to it, half way round the ſeed, as far as P Q N; ſo that in this part of the ſeed there is no meal next the huſk, but

only the leaf, though this is not the cafe throughout the whole of the feed; for if we cut it acrofs the middle, we fhall there find much meal next the hufk. From N the leaf proceeds inwards, as the letters N D S indicate; fo that the two lower parts of the leaf reprefented in *fig.* 25, at L and H, embrace or defend the whole of the future ftem, the external part of which ftem is fhewn in *fig.* 26, at the letters E and F. In this part of the ftem are plainly to be feen a great number of veffels, and likewife in the leaves themfelves may be feen many veffels, which here appear cut tranfverfely, and fome of them longitudinally.

The places marked by the letters T Q V and D O S, are thofe in which the meal is contained, and which it was impoffible to reprefent on the paper, therefore thofe fpaces are left vacant. This mealy fubftance confifts of globules larger than thofe in the meal of Wheat, Rye, or Barley; and thefe again compofed of fmaller particles or globules, fo that thefe laft are lefs than the globules in the before mentioned feeds.

But what appeared to me worthy of note, in regard to this mealy fubftance was, that the large globules I have mentioned, were not of a perfect fpherical form, but every one of a peculiar fhape; one of fix, another of eight or more fides. In fhort, I can not any otherwife defcribe their figure than by fuppofing a great quantity of globules of wax, or any other foft flexible fubftance, thrown promifcuoufly into a tub or box, and that, finking together by their own weight or foftnefs, and thereby almoft all the air which had been between them, while they were of a fpherical form, being excluded, they loft their fhape, and became compacted or fqueezed together. And thefe irregular figures they muft retain, until, being diluted in a fufficient quantity of fome fluid, they may have room to expand and refume their firft globular figure.

I have examined * many other feeds, and among the reft, a fmall feed called Manna, which is ufed to be boiled in milk, and which I remember in my childhood to have often eaten, it being efteemed a very wholefome diet. Concerning this feed, there is a vulgar report that it does not grow, but falls from the clouds in Poland at a certain feafon of the year, and that it is gathered in cloths before funrife, while the dew is on the ground. This notion I never gave any credit to, and having procured fome of this Manna, I faw upon the bare infpection of it, that we are much deceived refpecting it, for I could difcern in every grain, the place where the young plants had laid. But the true ftate of the cafe is, that before this Manna is brought to us, it is ground in mills, in order to ftrip off the external hufk, as is practifed with Rice, by which means, the feeds are broken in pieces, and the young plants they contain, for the moft part, deftroyed. I difcovered, however, many entire feeds, and in them the young future plant; from whence the futility of the above idle tale is apparent. But it may be true, that it is the practice to reap and gather in this feed early in the morning, by reafon that otherwife the heat of the fun might caufe the pods containing the feeds to burft open and fhed the feed.

Being once on a journey in Gelderland, where much Tobacco is planted, I defired to have fome of the feeds of that plant purchafed for me, having never feen any.

This is a very fmall feed, being no bigger than a large grain of fand. Upon placing one of them before the microfcope, I faw that its outer covering or fhell, was more curioufly formed than any feed I had ever viewed with my naked eye; and indeed it affords fo pleafing a fpectacle, beyond any other feed, that, had we fuch an object vifible by the naked eye, we fhould not hefitate to

* The author has given fpecimens of feveral other fmaller feeds, but as they are all reducible to one or other of the defcriptions before given, they are omitted in this tranflation, to avoid prolixity.

give it a place in a cabinet of curiofities; and, when ftripped of its outer coat or fhell, and of an inner covering or membrane, the beauty of it is ftill more confpicuous. To give a view of this pretty object, I caufed a drawing to be made of it from the microfcope, and this is to be feen at *fig.* 27, P Q R.

I alfo caufed a drawing to be made of the young plant taken out of the feed, as it appeared when viewed by the microfcope: this is fhewn at *fig.* 28, S T V W, wherein T V W is the part which would grow up into a plant, and W S T, the root. I had another of thefe young plants placed before a microfcope, of which I have alfo given a drawing at *fig.* 29, X Y Z A; and here again we may fee how this fmall production of Nature exceeds, in beauty, many of her larger works.

Having for fome time contemplated this feed, I took my little brafs box, which I filled with wet fcowering fand, and placed therein a few of the feeds: the box I carried * in my pocket in the day time, and at night placed it in bed befide me, that it might always be warm, and thereby the vegetation of the feed be expedited, as I had done before with other feeds; for I was defirous of feeing whether the young plants pictured at *fig.* 28 and 29, would agree in fhape with the plant when in vegetation.

Thefe feeds I examined every day; the firft and fecond day I faw no other alteration, than that they were fomewhat fwelled: the third day, I faw that the fhell was burft open, and at the lower part of the opening the plant had fprouted about two hair's breadth from the fhell. On the fourth day, I faw with great furprife, that the young plant was grown fix times as large as the original feed, but, as I could not then conveniently get a drawing made of it, I deferred it till the following day, when the young plant was become twice as long, as it was the day before. *Fig.* 30, H I K L,

* Mr. Leeuwenhoek means the pocket of his *broek*, (Anglicè breeches), which is made large, and being next the body always warm.

reprefents the two leaves which are fhewn in *fig.* 28, at T V W, and in *fig.* 29, at Y Z A.

And now I was not only convinced that, in diffecting the feed, I had formed a true judgment as to the different parts of it, but here I plainly faw the multitudes of veffels of which the leaves in great part were compofed; and of which in the two former fubjects, pictured at *fig.* 28 and 29, I had only feen, as it were, the fhadow. I could moreover diftinctly fee a great number of valves or joints in each veffel or tube, as well in the leaves, as in the ftalk and root, according to what we fee in the formation of larger plants.

While I was preparing this object for the limner to make a drawing of the root, as fhewn in *fig.* 30, at M O N, I faw fo many of the grains of fand adhering to it, that I was obliged to ufe feveral expedients to clear them away; and in doing this, many of the fmall branchings from the root were broken, by reafon that they had twined moft clofely round the grains of fand in a manner wonderful to behold; and if all the ramifications which I faw, had been reprefented in the figure, the number of radicles branching from the larger root, M O N, would have been twice as many as here fhewn: and what is more, in many of thefe fmall radicles, might be feen their exquifitely minute veffels.

The larger root, *fig.* 30, M O N, in a very little time became fo dried, that in order to make a true drawing of it, and to reprefent its veffels properly, I found it neceffary to place five or fix different plants before the microfcope one after another.

Now fince we fee that in fo few days time, this fmall feed will grow to a perfect plant, and moreover confider, that the fame perfection in its parts which we obferve in the plant, is contained in the feed, though hidden from our eyes, we are not to wonder that in a bird's egg, duly impregnated, a young bird fhall in a fortnight's fpace or lefs, be completely formed, equal in fize to the egg itfelf.

In like manner as in feeds, fo in the buds of trees the future produce may be diftinguifhed; for I have, in the middle of winter, cut off a twig from one of my beft bearing fruit trees, and on opening fome of the buds, and viewing them by the microfcope, I could difcover the bloffoms, though wonderfully minute. At the fame time alfo, I cut a twig from a currant tree, and opened the buds, wherein I faw, not only the fmall currants, which were inclofed in a double covering, and lay in as compact and perfect figure as fmall bunches of grapes, but I alfo faw the germ or firft fhoot of the future young wood of the tree, which was already formed and prepared to iffue from that part where the bunches of currants arife in the bud. *Fig*. 31, B C D, are two bunches of thefe currants feen through the microfcope, and E F G, is the germ or fhoot of the young wood.

Now, if we do not content ourfelves with merely examining the formation of different feeds, but weigh and confider in our minds, the wonderful and incomprehenfible perfection in the operations of Nature, or, more properly fpeaking, of the all-wife Creator, who in many feeds has created the future plant, with a mealy fubftance for its fupport and nourifhment, and in others has provided for the growth of the young plant, without any fuch fupport. When I fay, we duly weigh and confider all thefe things, we may conclude and be affured, that all the trees and plants now growing on the furface of the earth, have been produced in regular fucceffion from trees and plants of the fame fpecies which were created at the Beginning of the World.

On the Formation of Rufhes, and on the Structure of the Nerves.

IN purfuing my obfervations on the nature of the veffels or tubes of wood in trees, I turned my thoughts to the obfervation of Bulrufhes. Thefe grow in great quantities in the fhallow parts of our larger ftreams and rivers, and the feats or bottoms of our chairs are generally made of them ; and, as they have many cavities into which water will not eafily penetrate, they are ufed in time of war for the making of bridges to pafs troops over fmall ftreams or canals.

Plate XIX. *fig.* 32, A B C D, reprefents a piece of Bulrufh, which I cut at A, in an oblique direction, that the large cavities with which the infide of it is for the moft part formed, and which are vifible to the naked eye, may be feen.

Thefe large veffels or cavities in Rufhes, are furnifhed with valves through their whole extent, without which the Rufhes would be very brittle, and their fides eafily compreffed together; and likewife, were it not for thefe valves, if only one end of the Rufh was laid in water, the whole would foon be filled; but the water is by thefe valves prevented entering the cavities otherwife than very flowly, and the Rufhes, by reafon of the many cavities they contain, can bear, when floating on the water, a great weight in proportion to their fize.

I was alfo defirous to fhew this cavity in the Rufh when cut lengthwife, in order to place before the eye the valves as they are placed in each of thofe cavities. *Fig.* 33, E F G H I K, reprefents a fingle veffel, with its valves forming one of the cavities of the Rufh, which I fhall prefently fhew, as it appears through the microfcope, together with the nature of its formation.

I alfo placed before the microfcope two fides of one of thefe tubes or veffels in the Rufh, five of which generally form the cavity of the tube, in order, if poffible, to difcover the nature of their formation. This at firft appeared to me to be fomething fimilar to the bafket work, or hurdles, with which our country people fence their fields againft cattle, and which in other countries are placed on the outfides of houfes, and afterwards daubed over with clay. *Fig.* 34, L M N Q, reprefents one fide of this tube in the Rufh, drawn from the microfcope as nearly as the limner could copy it. Q N O P, reprefents another fide of the tube.

What appeared to me particularly worthy of note in this object was, that the before mentioned fides of the tubes were not compofed merely of the veffels running along thofe fides, and of which they feemed to be compofed, in like manner as I had hitherto conceived, that all the tubes of wood received their growth, or confifted of their own proper veffels; but the contrary here appeared to me, for I faw that each tube in the Rufh arofe from, or was compofed of, various peculiar fmall tubes.

In *fig.* 34, Q N reprefents a fmall tube in the fide of the cavity in the Rufh, to which I may not improperly give the name of a rufhy blood-veffel, and from which the fide L M N Q, in part proceeds, and from which alfo the fide Q N O P, is compofed or produced. This veffel, which I fo call a blood-veffel, and alfo all the veffels of which the fides of the large veffels in the Rufh are compofed, do again confift of many oblong parts, or rather of wonderfully minute veffels, from which veffels a great number of fmall veffels arife, and of thefe, the fides of the large tubes in the Rufh are compofed.

Thefe minute veffels, which I call blood-veffels, and are, as I have faid, compofed of ftill fmaller veffels, give rife to a great number of horizontal veffels, which take their courfe in as regular and exact order among the other veffels, as we may fee in the joints of the bamboo or reed from Japan.

In order to exhibit more clearly to view, the formation of this afcending veffel, which I call a blood-veffel, and from whence the fides of the large cavities in the Rufh proceed, I took one of the fmalleft of thofe veffels, the fides of which I tore afunder as exactly as I could, and placed them before the microfcope, that I might caufe as accurate a drawing as poffible to be made of them.

Fig. 35, R V, reprefents one of thefe fmall veffels, confifting of many ftill fmaller ones, out of which fmall veffels arife a great number of horizontal veffels, and of thefe laft veffels, as I have faid, the tubes of Rufhes are compofed, and thefe veffels as broken off from thofe tubes of the Rufh are pictured at S T and W.

This blood-veffel (as I call it), R V, from which fuch a wonderful production and fo many veffels arife is fo thin, that meafured by the naked eye, I muft fay, that it is not a twenty-fifth part the thicknefs of a hair.

But what feemed to me ftill more wonderful was, that I thought I faw from the afcending blood-veffels reprefented in *fig.* 34, at Q N, or *fig.* 35, at R V, and from the horizontal veffels which arofe from thofe blood-veffels, a matter or fubftance protruded, which immediately extended itfelf in length, and appeared to form a membrane. But when I examined this nearer, I faw that the fide of the Rufh's tube, Q L M N, was not a fimple membrane, but that each feparate part forming that fide was manifeftly hollow, and that the cavity was furrounded on all fides with a wonderfully thin membrane. This cavity is fhewn in *fig.* 35, about and before S.

Upon difcovering this, I placed the fides fo broken off from the tube of the Rufh, before a deeper magnifier, and directed the engraver to make a drawing of two of the parts, in which he could moft plainly diftinguifh the cavities; and thefe are fhewn in *fig.* 34, between X and Y.

Now if we fuppofe that all thefe cavities, and each of thofe whereof the fide of one of the tubes in a Rufh confifts, cannot be formed but of an incredible number of fmall veffels, in like manner

as we fee the fmalleft membranes to be in the bodies of animals, we muft again exclaim, " what inconceivable minutenefs is here!"

Moreover, I cut a Rufh tranfverfely, in as thin a flice as I was able, that I might the better conceive the formation of what I have called the afcending blood-veffels, and bring them into view.

Fig. 36, A B C D E F G H I K L M N O P, reprefents a fmall piece of Rufh cut tranfverfely. In E F K L S, is fhewn, as nearly as the engraver could reprefent it, one of the before mentioned valves or divifions which are fhewn in *fig.* 33, at E F G H I K, and, as I have before faid, oppofe the entrance of water and give ftrength to the Rufh. This formation confifts of an incredible number of veffels and mebranes, each veffel being in a contrary pofition from that adjoining to it. This valve or divifion has five diftinct fides, as at S E, E F, F K, K L, and L S, and thus (as I have before faid) almoft all the tubes in the Rufh are formed.

Now let us fuppofe one fide of the tube in the Rufh, which in *fig.* 34, is defcribed at L M N Q, to be the fame as the fide which is reprefented cut acrofs, in *fig.* 36 at L M; and moreover, that in *fig.* 34, N O P Q, is another fide of the fame tube, fhewn in *fig.* 36, at M N, and alfo, that what I have called a blood-veffel, and reprefented in *fig.* 34, at N Q, is the fame with *fig.* 36, where an oval hole is fhewn between M and O, and that this veffel is cut acrofs.

Having made thefe obfervations, and moreover, feeing that in various cavities of the large tubes of the Rufh, there were formed many membranes, efpecially when the Rufh began to be fo thin, as is fhewn in the partition of two of the tubes in *fig.* 36, between D E S R and D R Q C. I concluded that the fides of the large tubes in the Rufh were formed of the fmall afcending tubes, which I call blood-veffels, as follows:

From the afcending veffels placed between B P Q, the fides of the veffels P O, Q R, B C, B A, and P T, are in part formed; and alfo from the afcending veffel placed in R, in part is formed the fide of the tube, R Q.

Now when the nutritious matter is protruded from the veſſel placed in Q towards R, in order to form the ſide of the tube Q R, it meets the nutritive ſubſtance protruded from R towards Q, to make alſo the ſide R Q, and when this matter or ſubſtance, ſo protruded for the formation of two diſtinct veſſels, and one ſide of a tube, is formed in great abundance; it is ſo compreſſed on all ſides, that in the cavities of the tubes, it grows into irregular membranes, as is here ſhewn in *fig.* 36, between C R Q.

And this is alſo the caſe with the ſuperfluous matter protruded from the aſcending blood-veſſels (as I call them), in D, R, S, E, for the formation of the ſides of the tubes D R, R S, and S E, where alſo, from the compreſſed matter, the membrane placed between D E S R is formed.

This formation, namely, each ſide of the large tubes, being formed of two diſtinct aſcending veſſels, is neceſſary, for otherwiſe, the large tubes in Ruſhes would not adhere firmly. But as the ſides of the large tubes are made ſo compact, that little or no aperture can be diſcovered in them, ſo, on the contrary, the membranes, which, as I have ſaid before, are accidentally formed in the cavities of the tubes, are very irregular, and have in them many apertures, and alſo broken parts, ſome of which have a cavity in them, and their irregular formation ſeems to me, to proceed from ſuch extra nouriſhment not being ſufficient to form entire membranes.

But what ſeemed moſt worthy of note in theſe membranes was, that their external edge ſeemed a little thicker, and that, in ſo thin a ſubſtance as were theſe membranes, various ſtreaks could be diſtinguiſhed, which I conſidered to be veſſels. And indeed, I am perſuaded, that however thin and tranſparent the membranes might be, they were no otherwiſe formed than of veſſels joined together.

I have often, with great pleafure, obferved the texture of the Nerves, which are compofed of very minute veffels of an incredible thinnefs, and which, running along by the fides of each other, conftitute a Nerve ; the cavity of each of thefe fmall veffels is about two thirds its diameter; and in order to examine them I directed the fpinal marrow of three cows and one fheep to be brought to my houfe, that I might extract from thence the Nerves.

I confider it as a misfortune, that I have not been able to exhibit the cavities in thefe Nerves to others, but no fooner do I bring them before my sight, than in a very fhort time, even lefs than the fpace of a minute, they fo contract by evaporation, that the wonderful object vanifhes and cannot be reftored. And I not only faw the fize or circumference of thefe veffels, (fome hundreds of which go to the compofition of a nerve no larger than a hair of a man's beard), but fome of their cavities I could as plainly diftinguifh, as if we were to pierce many holes in a paper with a fmall needle, and hold them up againft the fun. And although thefe cavities, or the orifices of thefe veffels are fo wonderfully minute, I have feen living creatures in the waters, which could have moved and fwam about in them with freedom. In fhort, the minutenefs of fome things on this earth is fuch as feems to exceed all belief.

I cut fome of thefe nerves into fmall flices, or round pieces, each little or nothing larger than the hair of a man's beard, and wetting them, I placed them on a glafs to dry; when dried and viewed by the microfcope, I faw in them many fmall knobs or rifings, which I concluded to arife from hence, that the many fmall veffels of which a fingle nerve is compofed, became contracted upon the evaporation of the moifture they contain, and thence arofe in protuberances.

It often happened to me, that when I moiftened one of thefe fmall flices of a nerve after it had been dried, and looked at it through the microfcope, that I faw fome fmall particles in great numbers, fwimming about in the water: thefe particles I judged,

proceeded from out of the veſſels, for the knife with which I cut the pieces off was as ſharp as a razor.

I am ſenſible, that what I relate here will not be credited by ſome perſons, who are perſuaded that what I advance cannot be proved by experiments or obſervations: but theſe ſort of objections weigh little with me. I am indeed, by the vulgar, treated as a conjurer, and that I publiſh deſcriptions of objects which do not exiſt in nature, but we will leave theſe men to talk in their own way.

I was lately exhorted by a reſpectable gentleman to go on with my inquiries, notwithſtanding my * advanced age; foraſmuch as, (he ſaid) thoſe fruits which ripen in autumn, are by nature the moſt durable. I have, therefore, ſet down the obſervations I have, with no little pains, made in my decline of life. And, indeed, it is no ſmall trouble to purſue the inquiry into ſuch minute diviſions; and it is scarcely to be conceived how the ſmalleſt nerves can be divided into ſo many branches.

Now, though I cannot give a drawing of a nerve cut tranſverſely, to ſhew the figure it preſented to my eye, I will give a figure of one viewed ſideways. *Fig.* 37, A B C D E F G, repreſents a very ſmall portion of a nerve, viewed in this poſition; the part ſhewn at A G F is only a branch proceeding from the larger nerve.

In this piece of a nerve I could not only diſtinguiſh the fibres of which it was compoſed, and which perform the office of veſſels, but I alſo ſaw certain cavities in each of thoſe veſſels, and the particles or contents within thoſe component veſſels ſeemed alſo viſible to me. Having ſeveral times cut tranſverſely, as accurately as I could, ſome of theſe minute Nerves, and moiſtened them with water, I ſucceeded in exhibiting to the limner not only the ſmall veſſels compoſing the Nerve, but in every filament or veſſel, of which I have ſaid the Nerve is compoſed, we both of us ſaw an oblong ſtroke or line, which, in fact, was no other than the cavity or orifice of the veſſel

* The Author was at this time eighty-five years of age.

compreſſed or collapſed, as we find is the caſe with blood-veſſels, when the fleſh that incloſes them becomes dry.

Fig. 38, B C D E F, repreſents this ſmall Nerve, the multitudes of veſſels compoſing which are cut tranſverſely, and in which the lines or ſtrokes denote the cavities or orifices of thoſe veſſels. This Nerve is ſurrounded, in part, by five other Nerves, repreſented in the figure by G G G G G, but in theſe I have only repreſented their external coats or membranes, omitting the veſſels they contain within them. At four places in the ſame figure, namely, at B N C, C H I D, E K L F, and A F M, are repreſented particles of fat, with which particles I have ſometimes ſeen a ſmall Nerve wholly ſurrounded: but in lean animals I apprehend the Nerves are only ſeparated from each other by their membranes; whence it follows that the juices deſtined to form themſelves into fat, when diſtributed through the body, will inſinuate themſelves between the Nerves; and, in ſome part of this ſame collection of Nerves, of but a few hairs' breadth, I ſaw ſeven ſmall Nerves, and no fat between them.

After this, I purſued my diſſection of the ſpinal marrow, until I could diſcover the parts extended lengthways in it; a ſmall portion of which ſpinal marrow, as far as art can imitate Nature, I have cauſed to be repreſented in *fig*. 39, M N O P. I alſo made ſeveral tranſverſe ſections of this ſpinal marrow, and at length, with great pleaſure, I ſaw its component parts to be placed exactly in the manner repreſented in *fig*. 38, except that they appeared rather larger to my eye than here appears. Moreover, in many places I could ſee the light of the ſun through the orifices of the veſſels which I had cut acroſs, and I pointed out the ſame to the limner; but as the object, in all other reſpects, perfectly agreed with *fig*. 38, I did not think it neceſſary to make a freſh drawing.

While I was ſeparating the ſtrong outer coat which incloſes the ſpinal marrow, from the marrow itſelf, I ſaw many minute Nerves, and, in different places, take a courſe out of the ſpinal marrow; ſome of theſe were ſo minute, that what I had at firſt taken for a

fingle fmall Nerve I found to be five at leaft, and each of thofe five as minute as that reprefented at *fig.* 37, G A F. But I wondered to fee that thefe fmall Nerves, as foon as they iffued from the fpinal marrow, were inferted into the membrane that furrounded it, and became in a manner united to it. But when they again quitted the membrane, they feemed larger, and to be covered with a new membrane or coat. But in this place I found the Nerves, which I had cut off near their origin, fo covered with fat, and furrounded with fuch ftrong membranes, that I could not feparate them to my mind.

Thefe obfervations I afterwards repeated in other objects, for the moft part with the like fuccefs, and in fome of them I faw the cavities or orifices of the fmall veffels, compofing the Nerves, ftill more diftinctly; and, upon fhewing the fame to a learned friend who called upon me, he declared that he could diftinctly fee the fame.

Whoever wifhes to make fimilar obfervations, muft be careful in cutting acrofs fuch minute veffels, fo to hold his knife and to make the incifion, that it may not be in the leaft oblique. The knife alfo muft be very fharp, and the edge of it as thin as poffible, otherwife the cavities of the veffels will, in the cutting, be compreffed and clofed up.

** The Author's experiments and obſervations reſpecting the quantity of Air contained in Water and other Fluids.*

I HAVE ſeen an engine in form of a pump, deſigned to extract or pump out the air from water, but the operation itſelf I never ſaw. Since that time, I turned my thoughts to the making ſome ſmall inſtruments for this purpoſe, and from which I hoped to derive more ſucceſs, than from the pump I had ſeen in the hands of other perſons.

For this purpoſe, I took ſome very round ſmooth glaſs tubes, and of an equal breadth or bore throughout; that extremity, however, which ſeemed (if any) wideſt of the two, I prepared for the introduction of a piſton or ſucker.

Plate XX. *fig.* 1, A B C E D, repreſents one of theſe glaſs tubes drawn upon a reduced ſcale, for the part marked A B, was upwards of fourteen inches long, and B C, twelve; the cavity or bore was, throughout, nearly the ſeventh part of an inch in diameter. The cavity in the ſmall glaſs tube, which by the flame of a candle I had joined to the larger tube, was one ſixth part the ſize of that larger one, conſequently, the contents of the larger glaſs tube would be thirty-ſix times the tube C E D: this laſt tube, C E D, was two inches and two thirds of an inch long.

I then took a braſs wire, as *fig.* 2, G H I K, at one end of which, with a file, I made three notches, to which end, I faſtened a ſmall piece of leather, binding it on with fine ſilk, which ſilk was very

* This Eſſay, and the next following, though not pertaining to microſcopical ſubjects, are inſerted to ſhew the Author's diligence in his inquiries, and the accuracy of his obſervations in other branches of Natural Philoſophy.

firmly twifted in the middle at I: the other end of the wire I hammered flat, and faftened it to a fmall wooden ball.

Having thus prepared the tube, I filled it with fair rain water, and inferted into it the brafs wire by the end H I K, having firft well tallowed the leather bound round this end, and I preffed it fo forcibly into the tube, that no particles of water could pafs by it: indeed, fo violent was the preffure, that in this experiment, I broke four of my glafs tubes.

This glafs tube being thus filled with water, that no air could be feen at M, I preffed the inftrument L M, which I will call the pifton, flowly into the tube, until the lower part of it, O N, at the extremity N, came near to the oblique or curved part of the glafs. In doing this, the water iffued out of the aperture in the fmall tube D, in a ftream like a fountain, until there was no water in the tube, but from D E C B to N.

I then held the fmall glafs tube from E to D, to the flame of a candle, until the heat drove out the water from E to D, which being done, I applied the aperture D to the flame, whereby the glafs melted, and clofed the orifice.

Having thus managed the glafs tube, that there was no air (as far as appeared) between N B C E D, unlefs from E to D, being one fixth part of an inch, I poured a little water into the aperture A, letting it run down and fettle upon the pifton at O, to the intent, that if there fhould chance to be any aperture or unevennefs in the cavity of the glafs, water and not air might pafs by the inftrument N O, when ufed as a fucker.

After this, I gradually drew up the pifton or fucker out of the tube, and, while I was doing it, I faw, that not only the tube from E to C, immediately became empty of water, and was filled with air, but the water in the larger tube was depreffed from C towards B, for the fpace of three inches; and, during this operation, many bubbles of air arofe out of the water, and bubbled to the furface, and ftill the more when I fhook, or with a tap ftruck

the glaſs. At length, I ſaw no more bubbles of air than what aroſe from the bottom of the water. But what appeared to me in theſe obſervations to be moſt worthy of note is, that the bubbles ariſing from the bottom, were at firſt ſo ſmall, as to be ſcarcely viſible to the naked eye, and by reaſon of their ſmallneſs, they aſcended very ſlowly; but as they roſe higher, they gradually became larger, and aſcended to the ſurface with greater velocity.

I then again thruſt in the piſton with as much force as I thought the glaſs would bear, and in doing this, I ſaw that the whole ſpace of air from D E C, and three inches farther from C to B, was ſo compreſſed, that the ſubtile particles of air which probably had found their way into the glaſs, while I withdrew the piſton, had not only again eſcaped out of the glaſs; but that alſo the ſubtile particles of air mixed with the before mentioned common air from E to D, and alſo the thin particles of air from the bubbles which aroſe out of the water, had, by this violent preſſure, been ſo forced and driven out of the glaſs, that there was no more ſpace of air than about the ſize of two grains of ſand to be ſeen in D.

After this, I again very ſlowly drew out the piſton, and then again ſo many ſubtile particles of air got into the glaſs, that they filled the whole of the ſmaller tube and a ſpace in the larger tube, the extent of three inches from C towards B. Again I drew the piſton very ſlowly out of the glaſs, leſt by the too ſudden irruption of air, the glaſs ſhould be broken. For it had happened to me three ſeveral times, that by drawing out the piſton too haſtily, the glaſs tubes broke at C or D.

The pains I took to make theſe experiments, was for no other end than to diſcover, if poſſible, what was the quantity of air extracted from the water: this quantity, though to the eye it ſeemed conſiderable, yet in fact could be but little; for each of the air bubbles ariſing out of the water is much increaſed by being mixed with the particles of common air. Having taken the piſton out of the glaſs, I ſaw the water in the tube ſettle itſelf at P; ſo that ſuppoſing all the

before mentioned air to be in its natural ſtate, that which came out of the water could not occupy a larger ſpace than from P to F. Now the diameter or bore of the ſmall tube D E C, being only one ſixth part that of the larger tube B C, the bore of the larger tube is thirty-ſix times the ſize of the ſmaller one, as before has been mentioned.

The water in the larger tube from B to C, ſtood at the height of twelve inches, but I will only ſuppoſe it eleven inches, and, reckoning from this, I compute that all the air bubbles diſperſed through large collections of water, and extracted from it, do not amount to one two hundred and fiftieth part of the bulk of the water. And upon another computation made by me, I found that the bubbles of air extracted from the water did not amount to one two hundred and eightieth part of the water.

After this, I took boiled water and treated it in the ſame manner I have deſcribed as to rain water, but after thrice repeating my experiments, I could not pump out or extract any air from it; though, indeed, I once ſaw a ſingle bubble ariſe, but this, I rather concluded to have found its way through ſome inequality in the glaſs, or from ſome water not boiled, which by accident was in the tube before the boiled water was put in.

But becauſe many people may think that, in my experiments, I could apply very little force in glaſs tubes, in proportion to what may be done by others with larger tubes, pumps, or ſyringes, I think it right to demonſtrate how much force I could apply in my tube.

We will then take the diameter of the piſton, or bore of the tube. to be the ſeventh part of an inch, and ſuppoſing that with this piſton, I could apply a force equal to ten pounds weight, the concluſion is, that in a pump or ſyringe of an inch bore, a proportionable power muſt be applied, which we ſhall find muſt be equal to four hundred and ninety pounds, becauſe the piſton of this pump or ſyringe is forty-nine times larger than the other; and if the bore

of the larger ſyringe was two inches, the force applied muſt be equal to a weight of nineteen hundred and ſixty pounds, following the ſame rule of proportion. This proves the great power I can apply with my ſmall inſtrument, which I have made uſe of for many years. And thoſe perſons who are any wiſe ſkilled in the rules of geometry and ſtaticks, will eaſily ſee the truth of what I advance.

In further proſecution of my inquiries on this ſubject, I ordered the blood of a calf to be brought to me in a clean earthen pot: after it had ſtood ſeventeen hours, and the thin liquor which is called the ſerum of the blood, ſtood at the top, I put ſome of that ſerum into a glaſs tube, concluding that if any air was to be found in the blood, it would be in this part called the ſerum, becauſe the globules which give the blood its red colour, are heavier than the ſerum, and, conſequently, muſt contain a leſs portion of air.

This ſerum I treated in the ſame manner as I had done the water, and I ſaw, that in drawing out the piſton, a great quantity of air bubbles aroſe from the ſerum; theſe were ten times larger than thoſe I had ſeen iſſue from the water, and adhered to the glaſs like froth. At length, having quite extracted the piſton from the tube, which was done very gently, for fear of breaking the glaſs, I ſaw that all the air bubbles, except a few, diſappeared. This ſerous matter being thus without any preſſure, I ſaw that the ſpace occupied by the air which had iſſued from it, ſtood in the tube at the height of one inch, and I computed that it was one three hundred and ninety-ſixth part of the ſaid ſerum.

I repeated this experiment on the ſerum, which I took as carefully as I could, from the blood of a calf after it had ſtood only two hours out of the animal, and I found that the bubbles proceeding from it, were not ſo many by far, as thoſe in the preceding experiment, but of a larger ſize. The ſpace of air in the glaſs when the piſton was almoſt wholly withdrawn, was from C to B, four inches and two thirds of an inch. The piſton being very ſlowly taken out of the glaſs, I ſaw all the air bubbles vaniſh, and

the large ſpace of air which in the large tube occupied four inches and two thirds, had ſo eſcaped out of the glaſs, that in the ſmall tube, from C towards E, there only remained of it the ſixth part of an inch. So that, according to my computation, the air in this blood only occupied the one two thouſand three hundred and ſeventy-ſixth part of the bulk of the blood from which it was extracted.

I then went to a butcher's, at the time I knew he was about to kill a calf, in order to extract the air from the blood while it was yet warm; and for this purpoſe, I took with me my inſtruments, and alſo a glaſs veſſel two inches diameter, and eight inches deep, in order to catch the blood as it iſſued from the veſſels of the animal; which having done, I put ſome of it, as ſoon as poſſible, into my glaſs pump, and treated it in the ſame manner as before deſcribed, and I immediately ſaw ſeveral air bubbles ariſe, which when they came to the ſurface, became very large. I alſo ſaw the blood ſink in the glaſs tube from C towards B, ſo that the tube was filled with air to the height of five inches. Before I began my experiment, I had accurately obſerved what ſpace of air was in the ſmall tube when I had cloſed its orifice at D, by the means of fire. Upon drawing out the piſton as ſlowly as poſſible, I ſaw all the ſubtile air, which as I before mentioned, had filled the glaſs tube five inches from C towards B, had either eſcaped out of the glaſs or returned into the blood; for I could not perceive any the leaſt increaſe of air in the ſmall glaſs tube from C to D. I then opened the glaſs tube at D, and poured out all the blood as carefully as I could, and then put into it a freſh portion of blood, and repeated my former method of drawing out the air; and then I ſaw that the glaſs tube from C towards B, being about five inches, was filled with air, and that ſome air bubbles aroſe out of the blood, which were very large. After I had very gently drawn out the piſton, I ſaw that the air in the ſmall tube was increaſed by the addition of two air bubbles covered with ſome blood, neither of which bubbles

exceeded the ſize of a common grain of ſand. But whether this ſmall quantity of air came out of the blood, or was air in the water with which I had waſhed out the tube, before I put the ſecond portion of blood into it, I cannot determine.

I then gave directions that the glaſs veſſel in which the blood had been taken, ſhould be brought to my houſe, and after it had ſtood an hour, and the blood was yet warm, I poured ſome of that which was towards the ſurface and was now grown a little thinner, into the glaſs tube, and having ſtopped the orifice at D, I again repeated my operation of pumping, and I found that the air produced by drawing out the piſton, occupied a ſpace in the tube from C towards B, of almoſt four inches, and, from the moſt exact obſervation I could make, I found that the air in the ſmall tube was augmented only about $\frac{1}{4025}$ part of the quantity of blood. I then left the glaſs tube with the piſton extracted, and the orifice at D ſtopped, for ſix hours, at the end of which time, I ſaw that the laſt produced quantity of air was diminiſhed more than half.

After the blood had remained eight hours after taken from the animal, and there was a quantity of ſerum collected on the ſurface, I employed myſelf to extract the air from it. But becauſe there were eight or ten air bubbles in the ſmall tube among the blood, I could not make ſo exact a computation as before, but from the inſpection of my naked eye, I thought that there was rather a greater quantity of air iſſued from the blood.

When this blood had ſtood in the glaſs twenty-ſix hours, I took ſome of the ſerum off it, which I put into the glaſs tube and drew off the ſubtile air, which appeared in many minute air bubbles riſing out of the blood, and I found the tube from C towards B, filled with air to the height of three inches and three quarters, and drawing out the piſton as carefully as I could, I thought the air produced was $\frac{1}{792}$ part of the blood.

After the blood had remained forty-four hours in the glafs, I repeated the experiment of pumping, but I found fuch a fmall additional quantity of air produced, that it was fcarce worthy of being noted.

After twelve hours more had elapfed, I again drew off air from the blood, but I perceived no alteration nor any increafe in the air produced.

After the blood had ftood about five days and as many nights in my ftudy, I took fome of the ferum from the furface, and put it into a new tube, wherein no liquor had ever been put: the length of this tube was feventeen inches from B to C, and eleven inches from A to B.

This I treated in the fame manner as before, and perceived that the air bubbles arifing from the blood, and the external air which found its way into the glafs, occupied a fpace of five inches and three quarters from C towards B. During my operation, I attended carefully to the air bubbles as they arofe, and which grew larger as they afcended, as I have before obferved; and I faw fome of them which were larger than others, rife to the furface fafter, and fome of thofe larger ones afcended with five or fix times the velocity of the fmaller ones. Some of thefe larger bubbles frequently drove the fmaller on one fide, to make way for themfelves in their afcent; but I never perceived any of thefe air bubbles unite together, which I believe was only caufed by the vifcofity or tenacity of the ferum in which they were formed.

Hereupon I figured to myfelf the reafon why a larger air bubble fooner reached the furface of the liquor than a fmaller one. For, if the axis of a fmall bubble is as one, and that of a large one is as three, the proportion of a column, which the power of afcent in the fmaller bubble will overcome, is to the larger one as one to nine, and the bulk of the bubble whofe axis is one compared with that whofe axis is three, will be as one to twenty-feven, and confequently according to this proportion, the larger bubble will ex-

ceed the ſmaller in the velocity of its aſcent towards the ſurface. And as this is the caſe with air bubbles in their aſcent, the ſame will hold good in falling bodies of equal denſity, as in balls of different ſizes, diſcharged with equal force from cannon.

But to return to the ſubject, I drew out the piſton very ſlowly, and having obſerved the whole with the greateſt attention I was able, I found that the air produced in the ſmall tube from C towards D, by the operation, amounted to a ſeven hundred and twentieth part of the bulk of the blood.

I know it may be ſaid, that my method of making theſe experiments was not ſufficiently accurate, to which I can only anſwer, that I conducted them in the beſt manner I could deviſe.

I made many other obſervations which I did not note down, becauſe I think I have ſufficiently proved, that while the blood is in the veins, or while it remains warm after taken out, there is no other air in it, than that thin and ſubtile fluid which pervades not only our bodies, but the ſubſtance of all ſolid bodies.

Now if it be ſo, that the blood contains no other air than this ſubtile fluid or medium which can pervade all bodies, and that the quantity of common air found in blood after it has ſtood ſome hours, is ſo ſmall as not to deſerve notice, we ſhall reject the doctrine of many medical and chirurgical perſons, who, when they are not able to aſſign the true cauſe of a diſeaſe, ſay, that the blood is in agitation, or in a ſtate of fermentation, and the like. But I think we may more aſſuredly than ever, lay it down as a maxim, that every motion in the blood depends on the heart alone, and that we ought rather to ſay, the blood is too much condenſed, and cannot eaſily be driven through the ſmall veſſels, wherefore its circulation is not duly performed.

But when the blood is ſo much thickened that it cannot paſs through many of the ſmall veſſels, and thereby its courſe is obſtructed, and ſtill is driven from the heart with its uſual force, I think that by this means it may be made to burſt through ſome of

the ſmall veſſels, and thereby produce red ſpots on the ſkin ; and I have often thought whether ſomething like this might not be the cauſe of thoſe ſmall ulcers on the ſkin, which we ſee in the ſmall-pox. But theſe are only my own private ſpeculations, which I ſubmit to the judgment of thoſe who make theſe matters their ſtudy.

In farther proſecution of theſe experiments, I took rain water well boiled, and after it was cooled, I put it into a clean glaſs, firſt waſhing out the glaſs with the ſame water, and let it ſtand in my ſtudy uncovered for two days. On the third day, I placed the glaſs in the window, which fronted the North-eaſt, where it remained all day, the ſun ſhining bright with a cold wind from the North. The following day I took my largeſt glaſs inſtrument, which I rinſed out with ſome of this water, and then filled it with more of the ſame water, then cloſing the end of the ſmall tube, and applying my operation of pumping, I ſaw air bubbles riſe out of this water in greater quantity than I had ſeen in rain water not boiled, but taken out of the ciſtern. Having drawn out the piſton, I ſaw that when the ſubtle air had eſcaped, a greater quantity of common air remained in the ſmall tube than I had before ſeen in the water which was not boiled, and according to my computation, the quantity of air produced was a two hundredth part of the bulk of the water.

I requeſted a ſurgeon of my acquaintance, that when he ſhould bleed any perſon, he would ſend me ſome of the blood ; in conſequence of which, I received from him a veſſel with ſome blood which had been about half a quarter of an hour expoſed to the air before I received it. This blood was ſo thick, that in two eſſays to fill the glaſs tube with it, the air found its way into the tube, and it was not till the third trial that I could fill the tube with blood only ; by which means I apprehend that more air was introduced into the blood, than if I could at once have put it into the tube. Upon applying the operation of extracting the air, I ſaw large air bubbles ariſe out of the blood; but upon taking out the piſton, I found, according to the moſt accurate computation I

could make, that the quantity of air extracted was $\frac{1}{1274}$ part of the blood.

I was invited by another surgeon to be present when he should open a vein in one of his pupils; but upon this occasion the blood issued very slowly, and as it were, drop by drop. Some of this blood I put into a glass tube which had had rain water in it, and applied the operation of drawing out the air, which I found amounted to $\frac{1}{2058}$ part of the blood.

Now, since we see that blood, when it first issues from the veins, contains in it no air, we may be confirmed in the opinion of rejecting those sayings of medical men, who, when they are called to sick persons complaining of pains in this or that part, tell them, that the wind has got into the part, or that it is a windy complaint, and the like; whereas they ought rather to say, that the vessels in that part are obstructed, that the blood or juices have not their free course or circulation; and these kind of obstructions cause pain, and sometimes swellings in different parts of the body.

Defcription of an Invention by the Author, for illuftrating his Ideas refpecting the Effects of the Earth's diurnal Motion on the Clouds in the Atmofphere.

A FEW years fince, the eminent Chriftian Huygens, of Zuylichem, paying me a vifit, our converfation chanced to fall on the diurnal motion of the Earth; whereupon I produced to him a glafs globe, of my invention, which is reprefented in Plate XX. *fig.* 3. And, upon my putting it in motion, he was fo much pleafed with the effect, that I made him a prefent of a fimilar globe: and having fince frequently reflected on the fubject, I determined to publifh this my invention, as illuftrating my ideas of fome of the effects produced by the Earth's diurnal rotation on its axis, which the generality of mankind do not attend to.

I caufed fome glafs globes to be blown, about feven or eight inches diameter, and with a fmall neck or aperture. Having filled one of thefe with water, I took fome red fealing wax, reduced to a fine powder, and put the fame into the globe. I then took a fmall leaden bullet, which would pafs through the neck of the globe, and boring a hole in it, I fixed to it a thread, and paffed the fame through a cork fitted to ftop the opening of the glafs globe, the hole in the cork being of fuch fize that the thread, to which the bullet was fufpended, might ftick in the cork at any length required; then putting the bullet into the globe, I caufed it, by means of the thread, to hang at a fmall diftance from the bottom, ftopping the mouth of the globe clofe with the cork; then, with fome twine, I made a kind of net-work round the globe, twifting the pieces of twine together, to the length of about a foot beyond the neck of the globe.

I then placed the globe on a table, and, taking hold of the end of the pieces of twine, I twifted them round feveral times, which, upon lifting up the globe from the table, caufed it to fpin or whirl round. In this contrivance I purpofed to reprefent, by the leaden bullet, our earth; by the water, the air we breathe; and by the wax, the clouds floating in the atmofphere.

Now, in the circumrotation or fpinning round of the glafs globe, the leaden bullet, moving flowly, feemed as it were, fufpended in equilibrio. But the particles of wax, which, while the globe was at reft, lay round about the leaden bullet, now flew off from it, and difperfed themfelves as far as the infide of the glafs globe would permit them.

Then lowering the glafs globe, while it was yet in motion, I fuffered it to reft on the table, upon fome paper or a cufhion, to prevent the glafs being broke; and when the globe was fo placed, I faw the particles of wax, at firft in a confufed and irregular motion, but at length they all fubfided and fettled round the leaden bullet, fo as almoft to cover it.

Now in like manner, as by the motion of the glafs globe, the particles of wax which at firft furrounded the leaden bullet, were driven away and feparated from it, fo I am perfuaded that the clouds, by the diurnal motion or revolution of our earth, are kept fufpended in the atmofphere. And as, when the motion of the globe ceafes, the particles of wax fubfide, and collect themfelves round about the leaden bullet, fuch, I am of opinion, would be the effect if the motion of the earth were to ceafe; for that all the clouds, the watery parts of the atmofphere, and other bodies that have gravity, could not remain fufpended in the air, but would fall to the earth, and there remain.

Again, if while the particles of wax lie round about and cover the leaden bullet, the cork which ferves as a ftopper to the globe be taken out, and the thread which is fixed to the bullet, be fo lengthened as to fuffer the bullet to lie at the bottom of the glafs;

then, the thread be twiſted round, and the bullet lifted up from the glaſs, it will be ſeen by its motion on its own axis, to drive off the particles of wax: and hence alſo we may gather this concluſion, that the revolution of the earth round its axis, is wiſely deſigned to drive off the vapours and moiſture from its ſurface on all ſides.

A B C D E F, repreſents the glaſs globe as it lies on the cuſhion, and G H, are the pieces of twine gathered up and twiſted together. I, is the leaden bullet faſtened to the thread K L D, paſſing through a hole in the cork or ſtopper.

In the bottom of the globe at A, and round about the leaden bullet, are the particles of wax; but when with the hand at H, the whole is lifted up from the cuſhion, then by the twiſting of the twine, the globe will be ſwiftly whirled about, and the particles of wax be driven to the ſides of the globe, as at B F. But when the revolution of the globe is ſuddenly ſtopped, we ſhall immediately ſee that the particles of wax quit the ſides of the globe, and are hurried about in an irregular manner one among another, and then collect round about the bullet, where at length they ſubſide and ſettle.

In the next place, I remove the cork ſtopper and draw it on the thread D M N P, as far as the place marked N O M, and then take hold of the thread at P, with my finger and thumb, and gently twiſt it, having firſt laid down the twine G H, by the ſide of the globe, and then lifting up the thread N P, in like manner as I had before done the twines G H, ſo as to raiſe the bullet a very little from the bottom, we ſhall ſee how the bullet, in its motion, drives off from it the particles of wax.

Certain Positions laid down by the Author, respecting the Circulation of the Blood in an Human Body, with his Opinion respecting the manner of exhibiting the Circulation by the Injection of Quickfilver.

I HAVE heard it faid that the blood is circulated throughout our bodies fourteen times in the fpace of an hour; but no reafons have ever been affigned to me from whence this conclufion was drawn.

This fubject has been much in my thoughts, efpecially fince I have feen fo much of the circulation myfelf; and I have been confidering that the blood which is carried to the extremities of our feet, muft pafs through nearly three times the diftance from the heart as that which is circulated to the top of the head. For let us fuppofe the extremities of the feet, in any human body, to be four feet and an half diftant from the heart, we muft at the fame time reckon the top of the head, in the fame body, to be only one foot and an half diftant from the heart; and, confequently, the blood which paffes through the head may perform three circuits, while that which is carried to the feet is performing one. Hereupon I have confidered with myfelf, how to lay down certain axioms or pofitions whereby I might inveftigate this matter, upon fufficient grounds, without paying any regard to bare affertions; and only admitting this fingle fact, that the blood actually does circulate to and from the extremities of the hands and feet, as well as the reft of the human body.

Upon reflecting how fwift a motion the blood appears to have in the arteries, when viewed by the microfcope, though when exa-

mined without the help of glaſſes the circulation ſeems to be ſlow; I have, in my mind, divided a minute of an hour into ſeventy-two parts (inſtead of ſixty ſeconds, as is the common way of dividing time), and I reckoned each of theſe ſeventy-two parts or portions of a minute, to be of ſuch a length, that in it a perſon can diſtinctly pronounce a word of four ſyllables.

I then took an eel, about a foot long, and placed it before the microſcope, and, from the moſt correct obſervation which I could make, I judged that, in the ſeventy-ſecond part of a minute, the blood in one of the arteries, which was of a ſize to admit three or four globules of blood at a time to paſs through it, had proceeded the ſpace of a fifteenth part of an inch; therefore the blood, in one minute's time, paſſed over a ſpace of four inches and four-fifths of an inch; and this number being multiplied by ſixty minutes, it follows that the blood in this eel could be carried or driven forward 288 inches in the ſpace of an hour.

Now, let us ſuppoſe that the heart (which in eels is placed near the head) was in this eel diſtant from the end of the tail, where the fartheſt extent of the circulation is performed, 11 inches, it follows that the blood, in its circulation, muſt be carried twice this diſtance, being 22 inches, before it returned back to the heart; and dividing the before mentioned number, 288 by 22, we ſhall find that in this eel the blood circulated to the extremity of the tail and back to the heart, ſomething more than thirteen times in the ſpace of an hour.

In the next place, we will ſuppoſe that the ends of the blood-veſſels in the head, and in the fins next to the head, were in this eel one inch and an half diſtant from the heart, and, conſequently, that in theſe veſſels the blood only performed a circuit of three inches; it will follow, by the ſame mode of computation as before, that in theſe parts the blood might perform its circulation ninety-ſix times in the ſpace of an hour.

Laſtly, if we advert to the blood-veſſels in this eel, in and near the heart, where probably the blood is not protruded the fourth part of an inch before it is carried back to the heart, we ſhall find that in thoſe veſſels in and near the heart, the circulation may be performed an inconceivable number of times in an hour.

I have many times endeavoured to ſee the current and circulation of the blood in quadrupeds, which always live on land ; but I never could diſcover it: among birds, I found a ſpecies in which I could moſt plainly ſee the motion of the blood, but I could not diſtinguiſh the complete circulation, by reaſon of the great number of blood veſſels, which cauſed the part I viewed to appear red.*

Now, if we take for granted that in our own bodies the blood is carried through the arteries with the ſame degree of ſwiftneſs as in eels, and that the extremities of the feet or toes are diſtant from the heart in any human body four feet and an half, or fifty-four inches, making the blood perform a circuit of 108 inches, it will follow that the blood is carried to the extremities of the feet, and brought back to the heart, in the ſpace of an hour, only two times or circuits, and two-thirds of another circuit.

In the next place, ſuppoſing the blood-veſſels at the extremities of our fingers to be two feet and three-quarters, or 33 inches, diſtant from the heart, making a circulation of 66 inches, we ſhall find, by the ſame mode of computation as before, that here the circulation can be performed $4\frac{4}{11}$ times in an hour. By the ſame rule, in thoſe blood-veſſels in the breaſt, and other parts in the body, only twelve inches diſtant from the heart, the circulation may be performed twelve times in an hour; and in thoſe veſſels in the fartheſt part of the head, which may be ſuppoſed 18 inches diſtant from the heart, the circulation may be performed eight times in an hour.

* A cock's comb and gills, Vol. I. p. 90.

But, if we make a calculation on thofe veffels in the body which are within twelve inches diftance from the heart, and confider moreover how many veffels there are very near to the heart, and even in the heart itfelf, here we fhall find that the circulation may be performed in a very fhort fpace of time.

Upon the whole, it may probably be true, that in an hour's time there paffes through the heart fourteen times as much blood as the whole body contains: but it cannot be faid that all the blood in our bodies performs its circulation fourteen times in an hour, forafmuch as we have feen, from what is before laid down, that in many parts of the body this cannot be the cafe; and in particular in the feet, where it appears that the blood can, in an hour's time, perform no more than two complete circulations, and two-thirds of another.

I am aware, that many may object to thefe my pofitions, refpecting the circulation of the blood, and fay that I have feen it, in frogs and in fifhes, only at the extremities of their bodies, where the motion of the blood is exceedingly flow; but that, if it could be feen in the veffels near the heart, there its motion would be found to be much fwifter: and, in proof of thefe opinions, they reafon by analogy to what we obferve in the motion of a folid body in the air, fuch as an arrow or a bullet, which moves with great fwiftnefs when firft difcharged, but whofe velocity continually diminifhes: fo, fay they, the blood muft move with much greater fwiftnefs in the veffels next the heart, than in thofe veffels of the fame fize which are near the extremities.

To this I anfwer, that the motion of the blood in our bodies, or in the blood-veffels in fifhes, has not any analogy to the motion of a folid body in the open air; and in proof hereof I have caufed a figure to be drawn, which may be feen in Plate XX. *fig.* 4. A B C D E F, which we will fuppofe to reprefent a leaden tube, from whence various fmaller tubes branch forth, as at B G, C H, D I, E K, and F L, and that this tube and all its branches are filled with

ſome liquid, ſuch as water or blood, or any other fluid ; and in the larger tube let there be a piſton or forcer, as a A B. Now, I ſay, that if the liquid in this tube be compreſſed, the ſmall branch, B, which is next the forcer, will not endure a greater preſſure than the branch, F, which is the moſt diſtant from it.

And though the ſmalleſt tube, F, be a million times leſs than the largeſt one, the preſſure of the liquor will be the ſame in both. To illuſtrate this by another example. Let the diameter of this largeſt round tube be repreſented by a ſquare veſſel, *fig.* 5, A B C D E F, the meaſure of one of whoſe ſides ſhall be as 5, the ſquare of which is 25 ; then let the ſmall round tube be repreſented by a ſquare one, as at *fig.* 6, which call 1, being formed of one of the diviſions of *fig.* 5, then this ſmall ſquare tube will be to the large ſquare veſſel in the proportion of 1 to 25.

Now, let us ſuppoſe the large ſquare veſſel to be filled with ſome liquor, which is preſſed with a force equal to one hundred pounds, the bottom of the veſſel, A F E, beſides the weight of the liquor, will ſuffer a preſſure of one hundred pounds; but to preſs the baſe of the veſſel, *fig.* 6, H, with the like force, will only require a weight of four pounds ; and if ſo, the preſſure on the baſe H will be exactly proportioned to the preſſure on the baſe A F E, becauſe this is twenty-five times larger than H. And we may even ſay, that if only the weight of a grain of ſand was impreſſed on the part a A B, in *fig.* 4, there is no reaſon why this preſſure ſhould not be felt in all the ſmaller tubes at the ſame moment as in the larger one ; for, as the whole of the water or other fluid is to be conſidered as a ſolid body, there is nothing to prevent ſuch impreſſion being ſo felt.

Theſe reaſonings of mine will be very eaſily underſtood by any one who is acquainted with the principles of hydroſtatics, as laid down by Stevinus.

But if we ſuppoſe the part A, in *fig.* 4, to be ſome feet higher than F or L, it may be ſaid that the water in the ſmall tubes, F and

L, will be more compreffed than in the tube B, folely by reafon of the gravitation of the liquor, if B is placed higher than F and L; but if the liquor in thefe fmall tubes has a paffage into other fmall tubes, in which the liquor can rife to the height at Q, then the preffure on the liquor will be the fame in all the other tubes; for the preffure is the fame in every part of the tube as at its bafe, regard being had to the diameter or breadth of thofe tubes.

In a word, the current of the liquor will be of the fame fwiftnefs in every one of thefe branches.

Now, if we confider the large tube, *fig.* 4, A B C D E F, to be an artery, and out of it various fmall arteries and their branches arifing, as B G, C H, D I, E K, and F L, and that M N O P Q R is a vein into which the arterial blood is difcharged, we muft neceffarily fay that L M, K N, I O, H P, and G Q, are veins, becaufe G H I K L are the places where the blood begins to take its courfe backwards.

But the circulation of the blood will be very much retarded in the extreme parts of our bodies, when the limbs are cold; whence it comes to pafs that the blood-veffels in thofe parts are fo diftended that our hands and feet are fwelled, and therefore there cannot be fuppofed to be fo rapid a motion of the blood in the extremities, as in thofe veffels which lie in the more internal parts of the body, unlefs all the parts of the body are equally warm.

A certain Doctor of Phyfic, to whom, among other perfons, I had fhewn the circulation of the blood, told me that this circulation had alfo been exhibited to him by a chirurgical gentleman; and on my defiring to know how it was fhewn to him, he faid by injecting quickfilver into an artery, which circulated back again through a vein; but when I afked him how they were affured that one of the veffels in which the experiment was made was an artery, and the other a vein, he anfwered that they were not certain as to that point. I alfo afked him what was the fize of the vein in which the quickfilver, fo injected, was circulated; to which he anfwered, that

it was above a thoufand times larger than thofe veffels in which he had feen the circulation of the blood at my houfe.

Hereupon I told him, that in this cafe a perfon might very eafily be deceived; and that the veffel or veffels to which the names of arteries or veins were given, might, in my opinion, be either one or the other; for that I had myfelf, at times, feen various veffels in which I concluded the blood had performed its circulation, and which were fo large that they might be difcerned by the naked eye, but that, in thofe inftances, I had always been miftaken; for, upon a more clofe examination, I had found that thofe feveral veffels had arifen out of a branch of one and the fame blood-veffel. To illuftrate this by an example:

Let A B C D E F G, in *fig.* 7, reprefent a vein, whofe two branches are both cut off at B and E, fo that we do not know that each of thefe is a branch iffuing from the vein A. Thefe two veffels, B C D, and E F G, we find again united by the veffel E F, as the blood-veffels are often joined together; and out of thefe veffels, by fqueezing, pinching, or rubbing, we may extract any blood that might be left in them. Now it may very eafily happen, that in extracting the blood, or by the ftrong injection of quickfilver or wax, the valves (if any fuch there are in the veffels) may be broken, and then the quickfilver or wax, fo injected, may be forced through that part of the veffel marked B C F, and return from F to E; whereupon B C D will feem to be an artery, and E F G a vein, and it will be thought that the circulation is performed through C F. When veffels of this kind have formerly been viewed by me, I have found myfelf to be miftaken in fuch conclufion; for, upon farther examination, I have found (as before mentioned) that both the branches B C D, and E F G, arofe out of the fame vein, A.

Again, fhould we meet with an artery, as reprefented in *fig.* 8, I K L, into which we inject quickfilver until the whole veffel I K L is filled, fo that we may be certain that in this veffel the blood is brought back from K to L towards the heart; neverthelefs, we

cannot ſay that I K is an artery and K L a vein, becauſe in thoſe veſſels, which are viſible by the naked eye, the blood is not carried from and alſo back to the heart in circulation; for where I have obſerved theſe arteries to have a bend, I have often perceived that they were ſpread into branches, as is here ſhewn at L, and theſe again into ſmaller branches; ſo that at firſt ſight we might ſay that the circulation is performed in M N O, and that M P Q, N P Q, and O P Q, are veins.

I was formerly of opinion, that the blood which, in the arteries, is carried to the extremities of the body, did, in its paſſage, gradually loſe part of its thinner juices, by the continual pulſation of the blood forcing thoſe juices through the coats of the arteries. But, after frequently contemplating the circulation, I am aſſured that the coats of the arteries are ſo formed, as not to permit the leaſt particle of the blood to paſs through them, until thoſe arteries become divided into ſuch very ſmall branches, as give paſſage to no more than one, two, or three globules of blood at a time; at which places the circulation or return of the blood to the heart takes place; and here, through the extreme thinneſs of the coats of the veſſels, the finer juices of the blood may find a paſſage on all ſides for the nouriſhment of the parts adjoining. For were it otherwiſe, not only the external parts would want a due proportion of nouriſhment, but the circulation of the blood itſelf could not be performed to anſwer its intent; for the external parts of the body require the greateſt ſupply of thin juices, by reaſon of the great expence of moiſture they are expoſed to from perſpiration and otherwiſe. But if the blood was carried both to and from the heart in circulation in the veſſels, which are ſo large that we can inject quickſilver or warm wax into them, and are of ſuch a ſize as to admit two or three thouſand globules of blood, or more, to paſs through them at a time, and the coats of thoſe arteries being, as I conceive, ſo ſolid and impervious that they will not ſuffer the very fine juices to paſs through them, the conſequence would be, that in thoſe large veſ-

fels, the blood would be brought back to the heart without benefiting the body; whereas we may be aſſured that the blood is circulated through the veſſels for no other purpoſe than to diſtribute nouriſhment to every part.

If we cut open an artery or vein of any animal, out of which the blood is emptied, we ſhall find the inſide of it as clean and white as if no blood had ever been in it; which I think is ſufficient proof, that theſe veſſels are formed in ſuch a manner as to retain the thinner juices of the blood in every part, except where they are divided into the moſt minute ramifications. Nor will this ſeem ſtrange to us, if we conſider thoſe fowls which ſwim on the water, and there ſeek their food; for we know, that how long ſoever they remain in the water, or how often ſoever the water covers their feathers, yet thoſe feathers do not at all imbibe the water.

I have ſtripped off the very thin membrane, or internal coat of an artery, and placed it before the microſcope; when I ſaw, with admiration, that it was compoſed of an inconceivable number of exceſſively thin parts, interwoven one among another, and linked together in the manner of a net: and when I examined the other coat of the artery, which covered the laſt mentioned, I ſaw that the parts of which it was compoſed, were carried round about the artery; ſo that the ſtrength of ſuch coat conſiſted in its being capable of great extenſion and contraction in breadth, more than in length.

Seeing this, I concluded that every time the blood is driven from the heart, all the arteries expand themſelves, which muſt occaſion a pulſation throughout all thoſe arteries; and as by this means they are every time ſtretched beyond their uſual width, they are ſo formed and conſtituted as immediately afterwards to contract themſelves, which contraction promotes the propulſion of the blood in thoſe arteries, until it reaches thoſe veſſels we call veins.

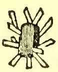
But the contrary, in my opinion, takes place in the blood-veſſel which we call a vein; for a vein, when emptying a part of its con-

tents into the heart, muſt neceſſarily at that time be contracted more than when in its natural ſtate, becauſe there cannot be a vacuum in the vein, and there is no ſubſtance at hand to occupy the place of the blood poured into the heart; for the blood cannot flow from the extremities, or even the parts adjoining, in ſo ſhort a time as would be required, whereupon muſt follow, as before obſerved, a more than ordinary contraction of the veins.

But thoſe parts, of which the coats of the veins are compoſed, which almoſt all take their courſe round about the vein, theſe, upon the effuſion of the blood into the heart, being put in motion, by their undergoing ſuch contraction as before mentioned, will, by their elaſtic tendency to recover their natural ſtate, draw the blood from the moſt diſtant parts towards the heart; ſo that this may be conſidered as a third cauſe of the conſtant circulation and current of the blood.

On the nature of Lime, and other kinds of Cement.

IN order to difcover the reafon why Lime and Plaifter, being moiftened with water, do, upon drying, acquire a degree of hardnefs like ftones, I took a piece of Limeftone, which, by being burnt, is rendered very foft, and divided it into very fmall particles; thefe I moiftened with pure rain water, and placed the mixture before the microfcope: I did not at firft obferve it to undergo any alteration; but, continuing my eye fteadily and inceffantly fixed, both on the fmaller and larger particles, I perceived, after ten or twelve minutes had elapfed, a faint appearance of exceffively thin and minute faline particles; at the end of fifteen minutes I faw them very diftinctly; and in half an hour I faw, not only in every particle of the broken Limeftone, but alfo in the water itfelf, fuch a multitude of faline particles as is almoft incredible. Thefe minute falts were not formed in ftraight lines, lying fide by fide, but for the moft part they croffed each other in all directions. For example: I faw a particle of Limeftone lying diftinct by itfelf; this particle was more than a thoufand times lefs than a grain of fand, and in it more than fourteen falts or faline particles were formed; they were not all of the fame fize, but fome larger than others, and alfo of different thickneffes; but the two longeft fides were all ftraight, and parallel to each other; the two ends, or fhorteft fides, were obliquely fhaped, yet parallel. This particle, with the falts which fhot or proceeded from it, as feen through the microfcope, are reprefented by the figure in the margin.



By the fight of this great quantity of falts lying together in this irregular manner, and efpecially where the particles of Lime lay

closely heaped together, I was fully satisfied as to the cause of the hardness or cohesion which Lime acquires after being moistened; for the salts shoot from it, one among another, in all directions, and being soft when first formed, they must, as they harden, become closely coagulated, and fixed to each other, so that not a single salt is formed in the substance, but it is united or connected with others: and I am fully convinced, that the hardness or strong cohesion we find in Lime when made into Mortar, arises only from the saline particles or minute salts produced in it, which are of such a nature, that when once formed, and thoroughly hardened and compacted together, they can never afterwards be dissolved, unless by the powerful effect of fire.

In reflecting on the nature of Lime, as thus described, I began to think that if the Mortar of old buildings were burnt over again, the saline particles formed in it would be so dissolved that it might be again moistened with water, and become serviceable as before. In order to make trial of this, I took some pieces of dry Mortar from the roof of my house, and heated them red hot; when cold I found they were become so soft, that they might be crumbled to powder between the fingers; and, upon wetting this powder with water, I found it to possess all the properties of new-made Lime.

Now, since we find that the saline particles in Lime or Mortar are the only reason that Mortar or Cement, when dry, becomes hard or stony, and that if the same be again burnt it becomes soft, all those saline particles being dissolved, insomuch that we can crumble it between our fingers, we may be well assured that Lime is unfit to make Mortar or Cement for walls of buildings wherein great fires are to be made, and that the proper substance for such kind of work is good Clay, which becomes the harder the more it is exposed to the fire.

I also concluded, that this shooting or formation of salts in common Lime, must also take place in that kind of Cement which is called Tras, or Terras, which is of that nature that it acquires a stony

hardneſs, even under water, and is therefore uſed in the conſtruction of water-works, and in other buildings where more than ordinary ſtrength and ſolidity are required.

This ſubſtance called Tras, is brought to us down the Rhine, and is ground to powder in windmills conſtructed for that purpoſe, and being mixed with common Lime, it is beaten or worked together a very long time, even for ſeveral days; for it is ſaid, that the longer it is beaten the better it is: and, in order to make work firm and ſtrong, it is cuſtomary to uſe three parts of this Cement or Tras and four of Lime.

A piece of this mixture of Lime and Cement, taken from an ancient building, I heated red hot, and when it was grown cold I found it ſo ſoft, that it was eaſily reduced to powder. This powder I mixed up with water, and perceived a great quantity of bubbles ariſe from it, the ſame as we obſerve in freſh Lime; after this had ſtood for an hour, it was grown ſtiff. I then beat it, or mixed it up again, and in a ſhort time it became exceeding hard.

This repeated beating, or working up of the Tras or Cement, is of very great uſe, becauſe, by this means, the air bubbles formed in it are expelled; for want of which being done the Cement cannot acquire its proper firmneſs. This appeared in the ſmall portion of Cement which I have juſt ſaid that I mixed or worked up only twice; for, after it was become dry, I broke it in pieces, and found in the middle of it many round cavities, which I concluded to have been cauſed only by the bubbles of air in it. But, when the Cement is beaten or worked together for ſeveral days, not only the air bubbles are expelled, but, after a certain time, no new ones are formed in it; and I am certain that if our common Lime was to be beaten or worked up for as long a time as the Tras or Cement, the bubbles of air which are formed in it would be all beat out, and the labour more than twice repaid by the goodneſs of the Mortar, eſpecially in caſes where ſolid and durable work is required.

Addition, by the Translator.

On the fame Subject; from Smeaton's Narrative of the Conftruction of the Edyftone Light-Houfe.

IN this work, Mr. Smeaton has given an account of his examination of all the fpecies of Lime or Cement that came to his knowledge; and he ranks among the firft, or moft powerful, the Terras defcribed by Leeuwenhoek; and alfo Puzzolana, which is a Cement of the fame kind, brought from Italy, as the former is from Germany. Both of thefe are fuppofed to be volcanic productions; and if fo, they are fubftances prepared by Nature, in the fame manner as Lime is by art, namely, the operation of fire. Mr. Smeaton recommends, for water buildings, the mixture of Terras or Puzzolana, with Aberthaw Lime, and he approves of repeated beating of the Mortar fo made, as it thereby acquires the greater ftrength.

Mr. Smeaton's fentiments on the preparation of thefe Cements, entirely agree with thofe of Mr. Leeuwenhoek, though our Englifh Engineer feems to have been ignorant of the caufe of the cohefion of Mortar, and of the reafon why it is improved by beating; which have been fo accurately invef-tigated and minutely defcribed by the Dutch Naturalift.

The Author's Difcoveries and Obfervations refpecting Wood confumed by Maggots.

CASTING my eye in the winter feafon on fome pieces of oak, (called in this country, *Tel-hout*), which I had kept in my houfe about fix years, for fire-wood, I faw that it was almoft wholly covered with a kind of white powder of the fame colour with the wood. Conceiving that this duft was caufed by fome maggots concealed within the wood, I took out of the heap a piece of wood very much covered with this powder; it had been cleft lengthwife, was about an inch thick, and eighteen inches long, and of five years growth; but I could not fee on the furface of it any of thofe maggot holes which we fee in many woods, although I had wiped off all the powder it was covered with. I then cut it in half, and having fplit the pieces, I took out of one half of it feventeen white living maggots, all of the fame fize; their bodies were fhort, and when taken out of the wood, they rolled themfelves up in a kind of circle: their fore part was twice as thick as their hind part: moreover, when taken out of the wood, they were not able to creep along; their feet were very fhort and covered with many hairs, and on each foot a ftraight claw. Thefe maggots had clofely compreffed the particles of wood which they had gnawed off, and alfo the excrements left behind them in the winding channels they had made in the wood, which I wondered they could do, becaufe their bodies were very foft, except the organs they had in the fore part of their heads, and thefe were not white, but of a yellowifh colour. Although I had no doubt that thefe maggots in the fummer time would be changed into flying animals, I neverthelefs put fourteen of them into a little box with a cover, which fcrewed on, and thefe I carried about me, to difcover how long they would live.

In the profecution of my obfervations on this fubject, I was aftonifhed to fee that the wood, on the furface of which there was no appearance of biting or perforation, was yet fo gnawed through, that it would eafily break. Now when people in general fee fuch pieces of wood, which on the outfide appear quite uninjured, and yet fo brittle, and contain fo much duft or powder, they think that the wood has a natural tendency to decay, or that it is confumed by maggots bred in the wood by fpontaneous generation, becaufe, as I faid before, they fee no holes in the furface.

But if we confider how fmall thofe eggs muft of neceffity be, from which fo minute an animal is produced, as proceeds from thefe Maggots by tranfmutation, and that a Maggot newly hatched from fuch an egg may creep into the chinks or cracks, or even the large veffels of the wood, and pierce holes in it fo fmall, as at firft to efcape the notice of our eyes, we may in fome meafure folve the difficulty.

I was well fatisfied in my mind, that the flying animals to be produced from thefe Maggots would be of the fpecies whofe wings are covered with fhells or cafes, and to fatisfy myfelf in this refpect, I took about a third part of the fame piece of wood (from which part I had not taken any maggots nor could I difcern any holes perforated by them in the furface), and inclofed it in a glafs tube, each end of which I ftopped with a cork; this tube I put in my clofet, in a place where it would be often expofed to my view, and that thus I might difcover what would become of the Maggots, if any there were, in it. This piece of wood I often looked at, but could not perceive any alteration, except that there appeared fome particles fcraped or bitten off the wood which lay in the glafs, and alfo a Maggot which had crept out of it and was dead.

On the 21ft of May, feeing no farther alteration in the wood, I took three other fmall thin pieces of the fame fire-wood, which I broke and took out of them about thirty Maggots, which were now changed into aurelias or cryfales, and as yet of a very white colour:

thefe cryfales were very prettily fhaped, for in them I could plainly fee their eyes, curioufly formed of various optical organs, having the refemblance of bunches of grapes; and I alfo thought that each eye confifted of more than two hundred fuch optical organs, befides which there were plainly to be feen the like joints, rings, or creafes as in the maggots, alfo the horns on the head, the feet, and the fhells or cafes for coverings to the wings; and, in a word, there appeared fo many and fo great perfections in this infignificant animal. that I could not view them without aftonifhment.

In about three weeks from this time, my occafions having called me out of town for a few days, I obferved at my return that all the animals which I had put into a box were changed into flying creatures, excepting two or three who were dead and their bodies dried up, and thefe I fuppofed I had injured in taking them out of the wood. I alfo faw eight flying animals, which had iffued from the fmall piece of wood I had put into a glafs tube, fitting upon the wood, and feveral round holes pierced in the wood, through which I concluded they had efcaped

Thefe flying animals were, in my judgment, fix or eight times larger than that which is produced from the maggot which feeds on the oily fubftance of Mace, of which I have treated at large in another place. The fhells or cafes alfo, which cover the wings, are nearly of the fame fhape in both fpecies.

From hence we fee that fuch young fhoots of oak, as I have before mentioned, are more liable to be confumed by maggots than the larger pieces growing in this country, and much more ftill than thofe which grow in warmer regions: and for this there can be no other reafon affigned, than that thofe fmaller pieces of wood, which are the fhoots which fpring from the roots of old timber trees, and grow up into faggot wood, are in this country very flow in growth, and confequently very porous; whereas the pieces or fhoots of oak which are of quick growth are firm and folid, and thefe maggots

can make but little impreſſion on them, and the reaſon * of this ſtrength and firmneſs in oak timber has been explained by me in another place.

Moreover, thoſe pieces of wood which are left with the bark on, are more obnoxious to be injured by maggots than thoſe which are ſtripped of it, becauſe, as I conceive, the maggots, while young, are more able to gnaw into the bark than into the wood itſelf; but when they have fed upon the bark for ſome time, they by degrees grow ſtronger, and are better able to gnaw the wood: of this we have a proof in baſkets, which are much leſs liable to be injured by theſe maggots when made of white wands than of thoſe which have the bark on.

* Vol. I. p. 5 and 6.

Deſcription of a minute Fiſh found adhering to the ſhells of Sea Fiſh: the Author's Examination of the Eyes of Flies and other ſmall Animals, and his Obſervations and Concluſions thereon.

I HAVE often taken notice of certain knobs or riſings which ſometimes are ſeen in great numbers on the ſhells of muſcles, though on other ſhells none are to be found. I have alſo ſeen them on oyſters, and I have counted as many as fifty on one ſhell: theſe knobs or ſwellings, are called by our common people, puſtules, or pimples. They are conſidered by many to be only caſual excreſcences on thoſe ſhells; but, if we examine them carefully, we ſhall find, that they are in reality ſmall fiſhes fixed to the ſhells of muſcles or oyſters, where they gradually increaſe in ſize; ſo that I once ſaw on an old muſcle one of them, whoſe diameter at the baſe was three quarters of an inch, and there were many other ſmaller fiſhes of the ſame ſpecies, but of different ſizes round about it.

Theſe ſmall fiſhes, which I call Puſtule-fiſh, or Pimple-fiſh, are fixed by their ſhells on different parts of the oyſters or muſcles. But whereas, oyſters and muſcles, when ſeeking their food, open their ſhells a little, theſe ſmall fiſh are incloſed by a ſolid ſhell on all ſides, except an opening at the top or point of each ſhell, in which opening are placed two very minute oblong ſhells, not fixed to the other ſhells, but to the fiſh itſelf, and by the help of which it can cloſe the opening in the other ſolid ſhell. Theſe two minute pieces of ſhell have each of them a ſtraight ſide, exactly fitting each other; the other ſides are rounding and ſloped to a point.

Upon putting ſome muſcles, to which ſeveral of theſe ſmall fiſh were fixed, into ſalt water, I ſaw the fiſhes protrude theſe ſmall ſhelly parts beyond the aperture, and open the extremities of them about as wide as the back of a common knife, and upon my touching thoſe parts lightly with a ſmall needle, the fiſh immediately cloſed them, and alſo drew thoſe ſhelly parts within the ſolid ſhell

which thus became ſhut. Theſe ſmall fiſhes lived only two days while in my poſſeſſion, but how long the muſcles to which they were fixed, had been out of the water when I got them, I do not know.

I have often taken theſe minute fiſhes as gently as poſſible out of their ſhells, and ſpreading their parts aſunder, placed them before the microſcope, and I muſt ſay, that I never with my naked eye, beheld any fiſh, in the formation of which there appeared ſuch wonderful art, as in theſe minute fiſhes ; therefore I determined to give a drawing of the greateſt part of one of them.

Plate XX. *fig.* 9, A B C D, repreſents part of a muſcle ſhell, in which, at E, is ſhewn the ſhell of this ſmall fiſh, as faſtened to the muſcle ſhell, and which, as I have ſaid before, is called a Puſtule, or Pimple.

The wonderful and curious formation of this little fiſh, the limner could not repreſent exactly after Nature, becauſe the beauty and regularity of its ſhape were loſt when the moiſture evaporated from it ; and beſides, the ſeveral parts of its body were ſo exactly and delicately joined together, that when I endeavoured to ſpread them aſunder, ſome one part or other was always broken.

This fiſh is provided with a receptacle containing a great number of eggs, but which, at the leaſt evaporation of the moiſture, are ſo contracted, that nothing of them can be diſtinguiſhed.

Fig. 10, A B C D E, repreſents the body of this fiſh as ſeen by the microſcope. E F, is one of twelve organs or limbs with which it is provided, beſides the other organs marked K L M, N O, and P Q R. All theſe organs lie bent in a kind of curved poſition, and are ſo twiſted one within another, that they cannot be diſtinguiſhed unleſs they are gently ſpread aſunder.

I have ſaid, that the eggs were ſo ſoft, that on the leaſt evaporation they loſt their ſhape and vaniſhed from the ſight, therefore I determined to boil ſome of theſe Pimple-fiſh a little, and by ſo doing, the eggs acquired ſuch a degree of hardneſs, as to keep their figure, and enabled the limner to ſee them diſtinctly and repreſent them in his drawing, as may be ſeen in *fig.* 10, at G H I. But I directed,

that only a few of the eggs ſhould be repreſented, for if all thoſe eggs which lay about that part of the body marked A B C, had been inſerted in the figure, the number would have been ten times as many as here ſhewn: moreover, it is to be obſerved, that all theſe eggs were held together by a kind of membrane, and in ſuch exact and regular order, that it was a pleaſure to behold them. On farther examination, I perceived in each egg a round corpuſcle like a yolk, and each egg was compoſed of a great number of larger and ſmaller globules, as I have always obſerved all eggs, or at leaſt the yolks of them, to be formed.

I have given a drawing of this minute animal, for no other reaſon, than to convince thoſe perſons of their error, who abſurdly maintain, that a living creature can be generated from corruption, by ſhewing the wonderful formation of ſo minute and contemptible an animal, and which not one man in a thouſand knows any thing of, not to mention that many veſſels, nerves, and organs, worthy of admiration, may be ſeen in this ſmall creature, when firſt taken out of the ſhell, and while the parts of its body remain moiſt.

I have often made repeated diſſections of the eyes of various kinds of flying inſects, merely on account of the pleaſure the contemplating them afforded me.

In particular, in the ſummer ſeaſon, I examined the eyes of that large flying inſect called the * Dragon Fly. Theſe being placed on a ſheet of clean paper, with a ſmall hair pencil and fair rain water, I cleared away the many veſſels which fill the inſide of the tunica cornea, or horny coat of the eye, leaving only the tunica cornea remaining. This I contrived to place in ſuch a manner, that it might not, as it dried, contract in wrinkles, and placed it before the microſcope, and I often contemplated it with great admiration, for I not only ſaw the many parts or optical organs of which it was compoſed, each of which was a perfect portion of a ſphere; but each of theſe ſpherical parts was incloſed in a partition ſeparating

* See page 47, of this volume.

it from the others, exhibiting at the parts of feparation a kind of luminous appearance, fo that each fpherical part was furrounded by fix fides, as is fhewn in *fig.* 11, A B C D.

When I removed this tunica cornea a little from the focus of the microfcope, and placed a lighted candle at a fmall diftance, fo that the light of it muft pafs through the tunica cornea, I then faw through it the flame of the candle inverted, and not a fingle one, but fome hundreds of flames appeared to me, and thefe fo diftinctly (though wonderfully minute) that I could difcern the motion or trembling in each of them.

Directing my view, through the fame tunica cornea, to the fteeple of our new church, which I have meafured by a quadrant and found to be two hundred and ninety-nine feet high, and I judge to be about feven hundred and fifty feet diftant from my ftudy, I faw the reprefentation of a great number of minute fteeples inverted, and which feemed no larger than the point of a needle feen by the naked eye.

When I looked at the neighbouring houfes, I could fee, through the tunica cornea, not only the buildings, but the doors and windows, and could plainly diftinguifh whether the windows were fhut or open.

It muft be obferved that in *fig* 11, A B C D, a very fmall part only of the tunica cornea is reprefented, but to give an idea of the whole of it, and to fhew the multitudes of the protuberances or optical organs it contains, every one of which is a perfect eye, I counted how many of thofe protuberances were contained in the diameter of the whole tunica cornea, which amounted to about one hundred; but if we reckon them to be only ninety, the number of optical organs in the whole furface of the tunica cornea will amount to above eight thoufand.

In the fame manner I alfo examined the eyes of a fly, and I perceived the fame perfection in the formation of the optical organs it contained (though thefe were fmaller than in the dragon-fly); and

through every one of them I could moſt plainly diſtinguiſh all ſurrounding objects.

Fig. 12, E F G, repreſents a ſmall portion of the tunica cornea in the eye of a fly, in which thoſe protuberant parts or optical organs were not fewer in number than in the eye of the dragon-fly. And to ſhew the incredible perfection of each of theſe optical organs, I took a large grain of common ſcowering ſand, and I muſt ſay, that above a thouſand of thoſe protuberances, in the eye of a fly, taken together, were not equal to the ſize of that grain of ſand.

What I have here ſaid, reſpecting the eye of a fly, muſt alſo be underſtood of the eye of a gnat, regard being had to the comparative ſmallneſs of the gnat's eye.

About the time of my making theſe obſervations, I found it aſſerted in the writings of a certain author, when treating of ants and moths, that ants have no eyes in their heads, on account of their ſmallneſs; but, meeting with ſome ants in my garden, I found, upon diſſecting them, that each of their eyes contained fifty optical organs; and in the brains of thoſe creatures I ſaw, with admiration, the wonderfully minute blood-veſſels they contained.

Reſuming the examination of the eyes of the dragon-fly, I was deſirous to know whether the tunica cornea in them conſiſted of many ſcaly particles laid one on another, as I have in another place deſcribed to be the formation of it in the eyes of men and quadrupeds. This diſſection, after ſome difficulty, I accompliſhed; but I obſerved that the leaſt alteration made by diſſection in the tunica cornea, prevented my diſtinguiſhing objects through it: I, however, diſcovered that it was in reality compoſed of many particles or ſcales, laid one on another, of which I could diſtinctly count thirteen ſeries.

Now, if we conſider the multitude of parts of which theſe ſcales or ſkins muſt be formed, which eſcape our reſearches and our ſight, and moreover, how cloſely and exactly they muſt be laid together, to give free paſſage to the light, we ſhall be loſt in wonder at ſuch perfection in the eyes of ſuch inſignificant creatures as theſe flies:

at the ſame time we muſt admire the regularity of Nature's works, all whoſe operations are performed in ſimilar ways, both in the formation of the eyes of theſe minute creatures, and in thoſe of larger animals, and alſo of the human ſpecies.

Now, ſince we ſee the regularity of the propagation of the minute marine animal I have been deſcribing, and when we contemplate the wonderful formation and incomprehenſible perfections in the eyes of flies, and the admirable conſtruction of their many optical organs—when, I ſay, theſe things are known, ſurely none will be ſo abſurd as to retain the notion that any animal, however contemptible in our eyes, can be produced ſpontaneouſly or bred from corruption.

But I will conclude with this wiſh, that the eyes of all may be opened to the truth of the regular propagation of all the animals and vegetables on this earth; and to admire the infinite wiſdom of the Creator, in the formation of all things at the Beginning, and in the wonderful and infinitely diverſified proviſion made for their propagation through all ſucceeding ages of the world.

END OF THE SECOND VOLUME.

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Plate XI.

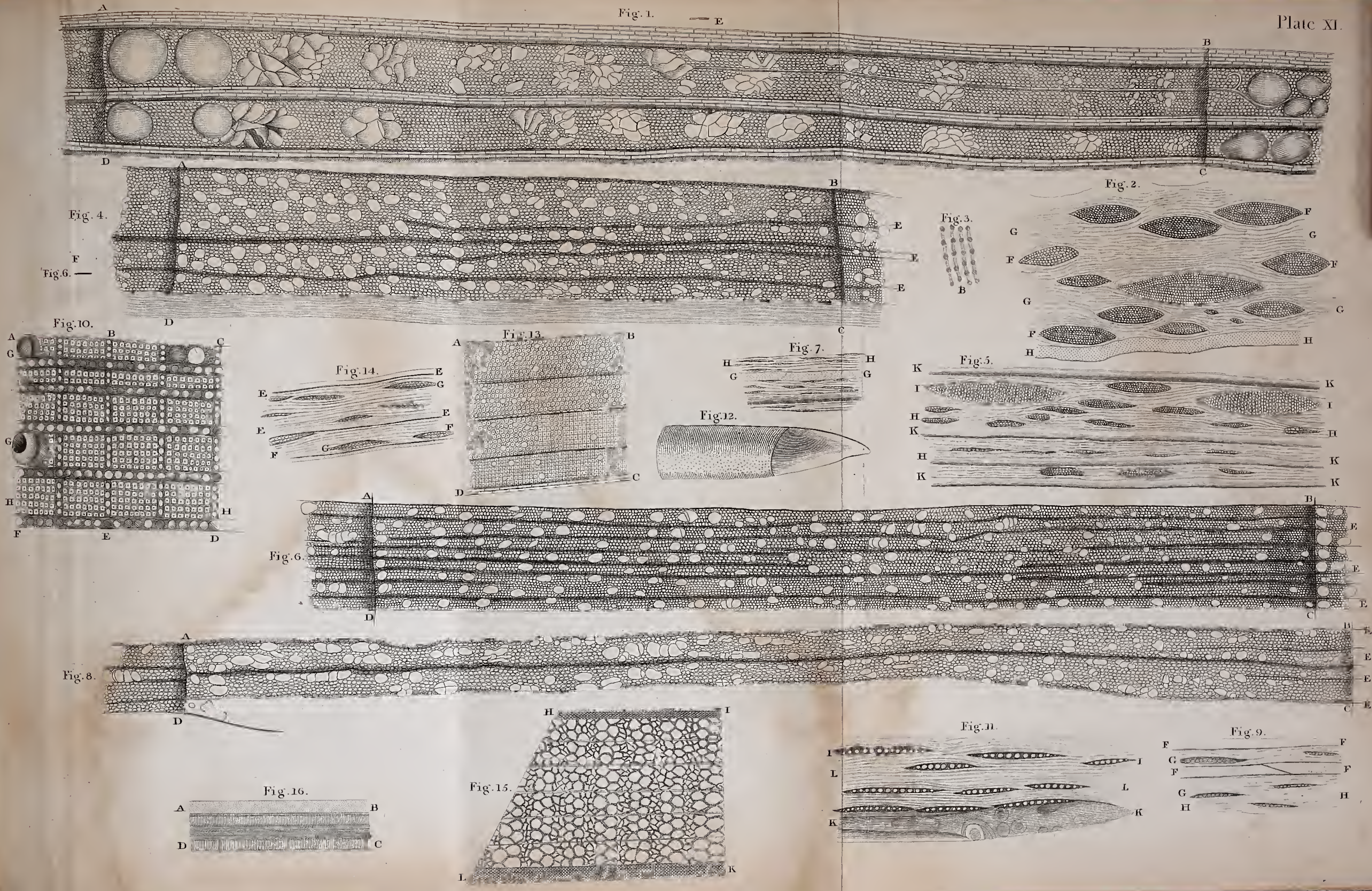
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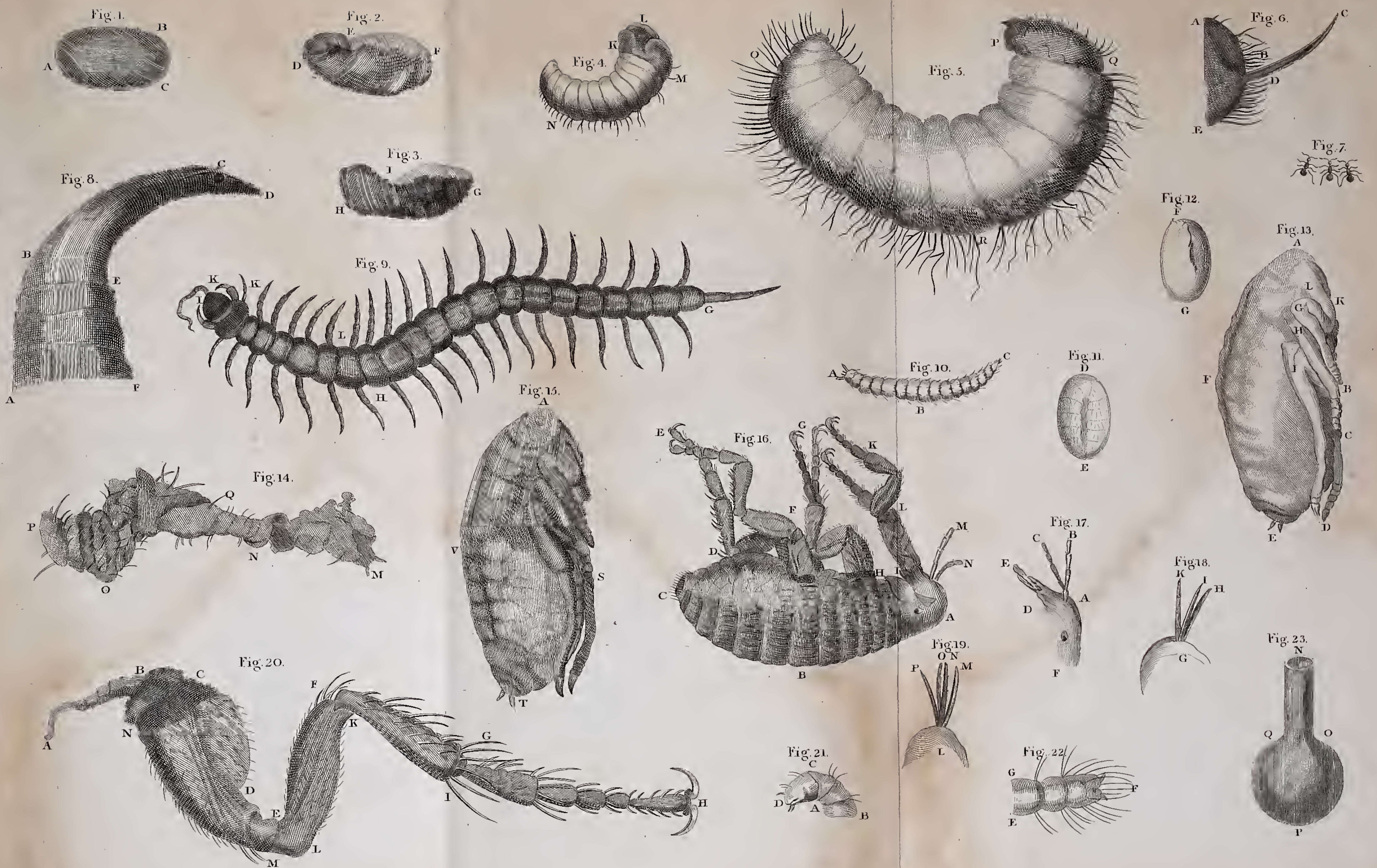
Plate XII.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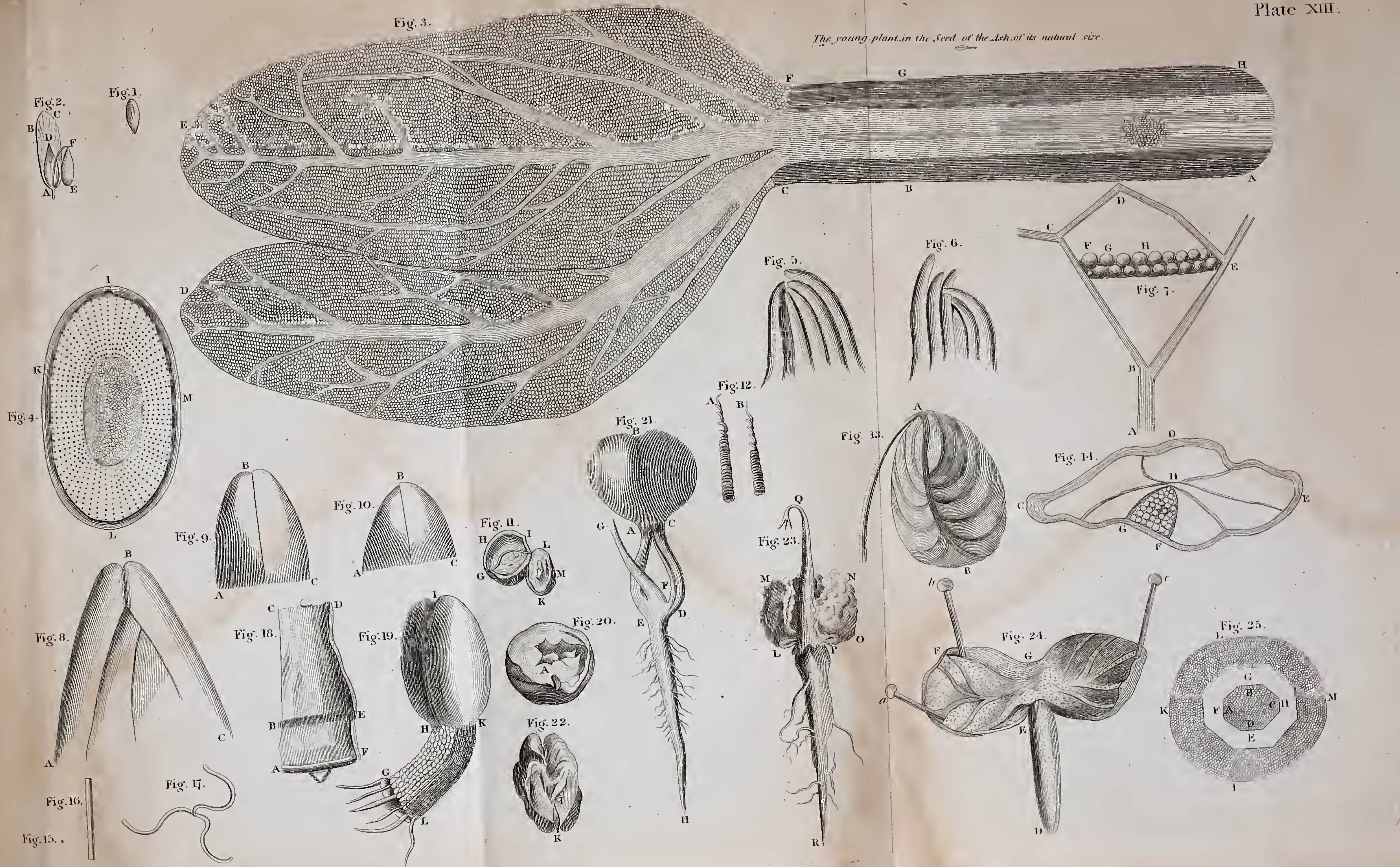
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Plate. XIV.

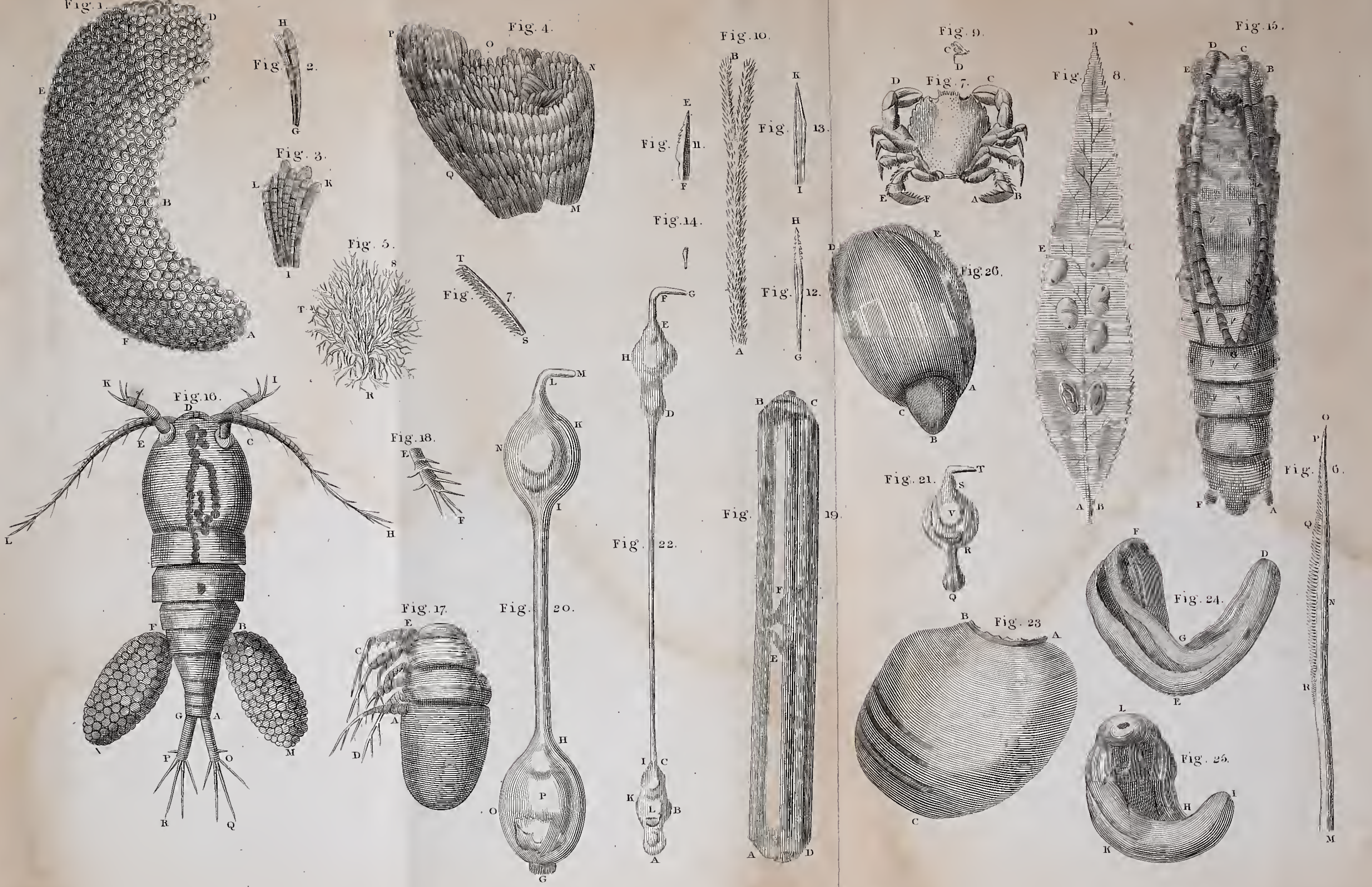
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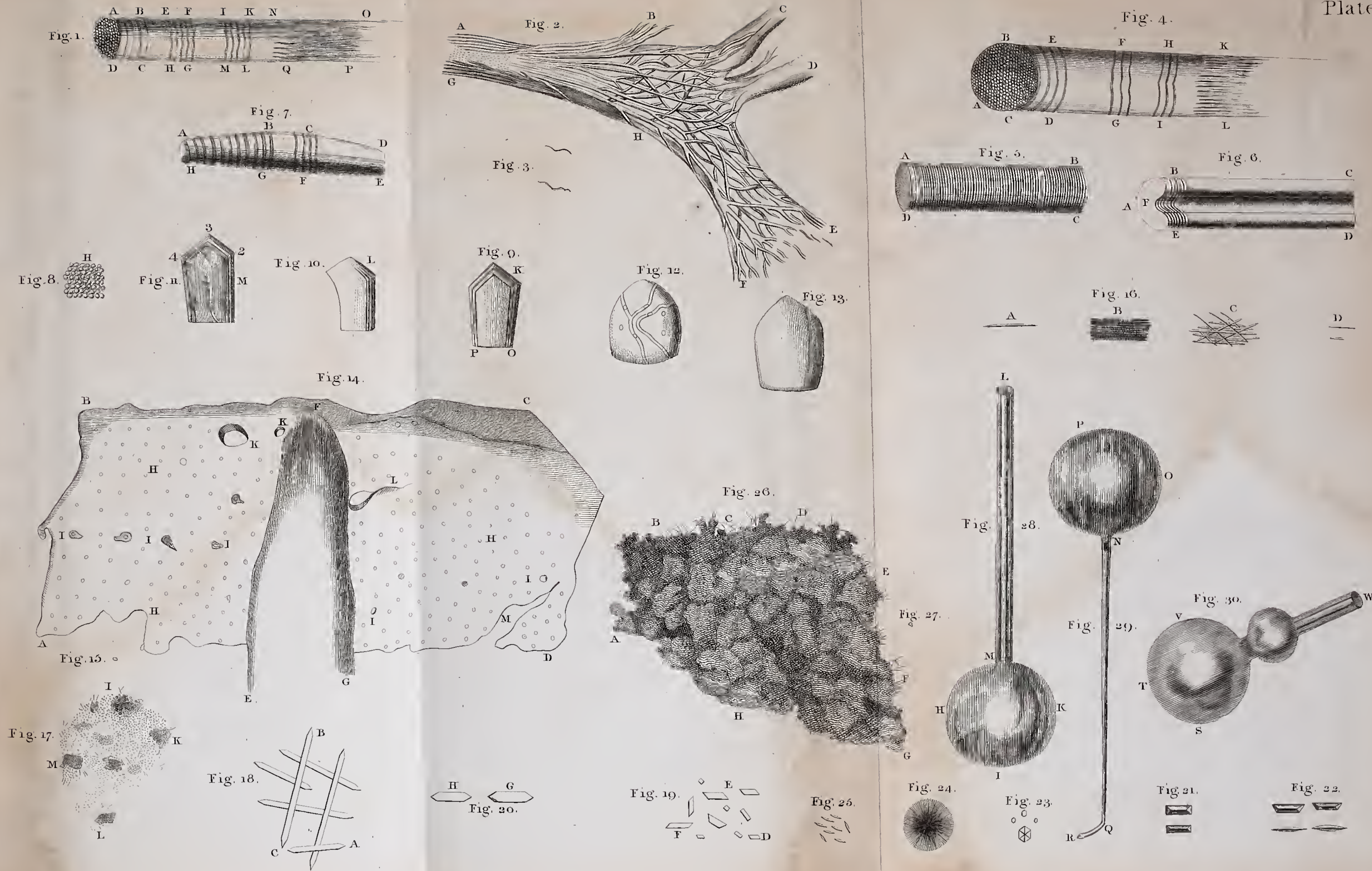
Plate XV.



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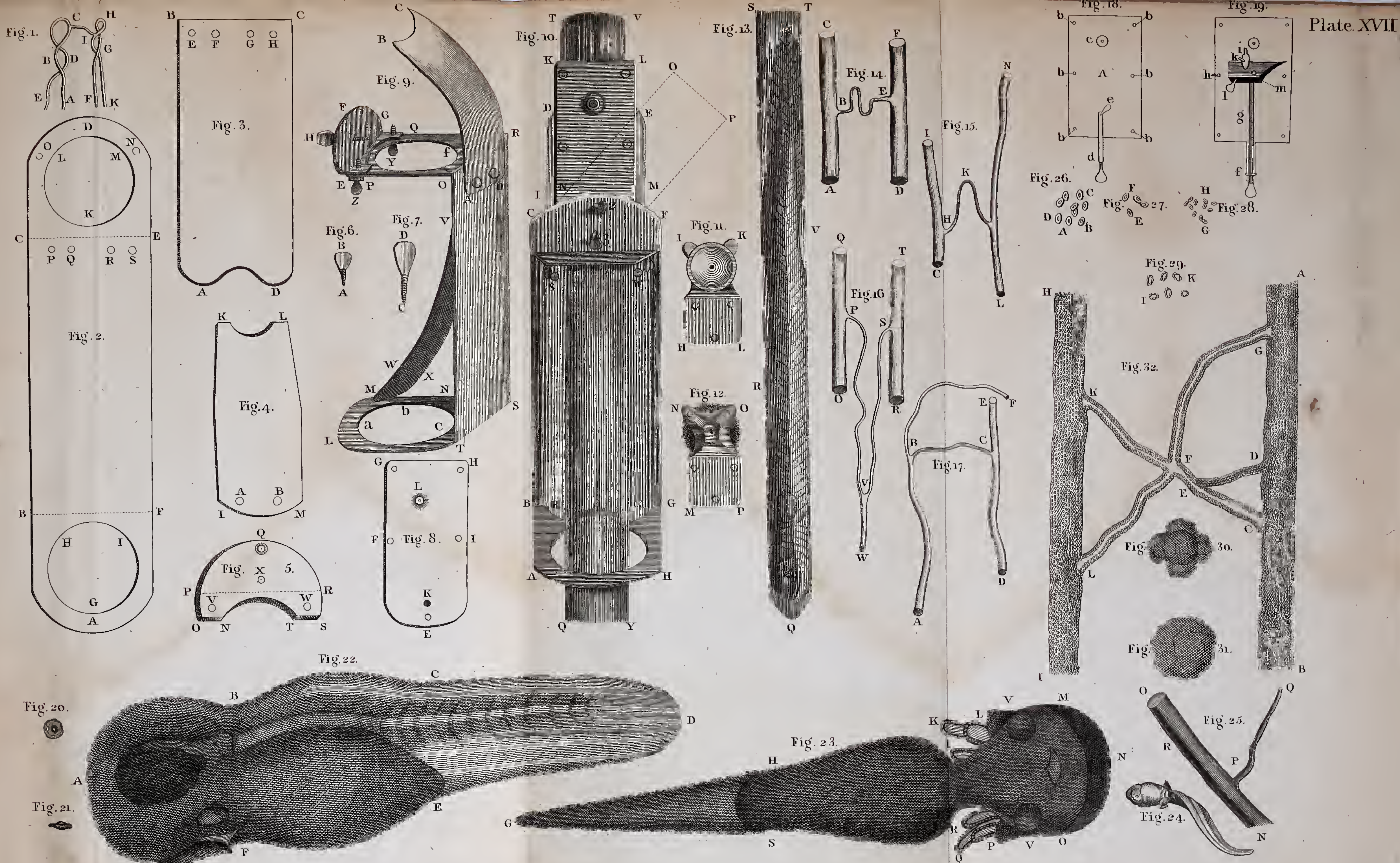
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Plate. XVIII.

See Plate XX Letters a.b.c.d.e for the five first figures omitted on this Plate.

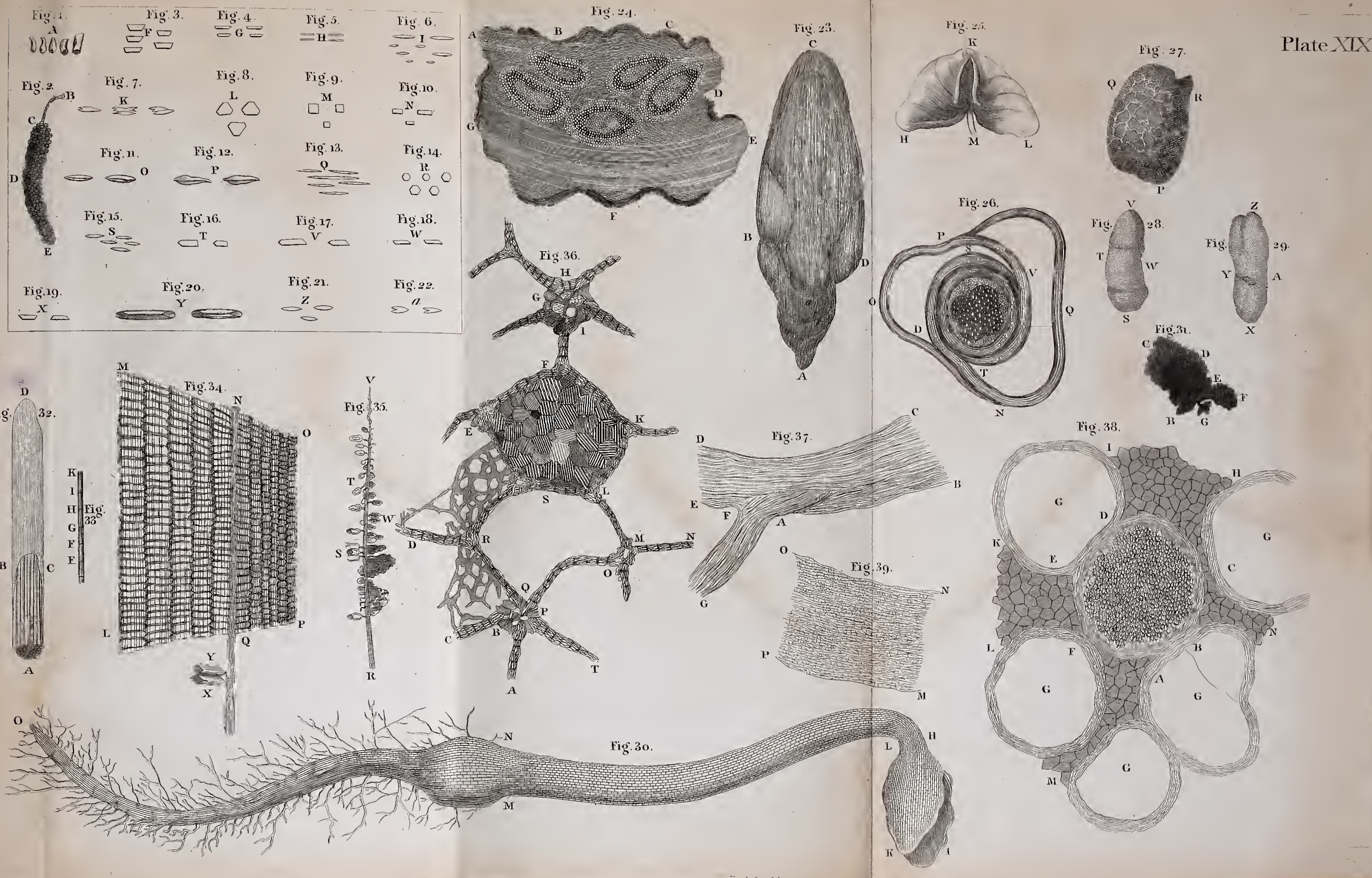
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Plate XX.

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